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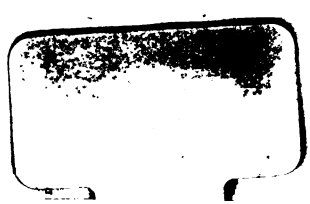

# BRAITHWAITE'S RETROSPECT.

VOL. LXIII. JANUARY—JUNE, 1871.

THE

# RETROSPECT OF MEDICINE:

BEING

## A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.


EDITED BY

W. BRAITHWAITE, M.D.;

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D. LOND.



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# SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

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## AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER.—*Hydrate of Chloral.*—Hydrate of chloral, in doses of ten to twenty grains three times a day, relieves the pain of cancer without interfering with the digestion and producing vomiting, as opium not unfrequently does. (Mr. W. Cooke, p. 326.)

EPIDEMICS OF CHOLERA AND FEVER INCOMPATIBLE.—Mr. Lawson, Inspector-General of Hospitals, has observed in India that epidemics of fever have an influence in checking the advance of those of cholera, and *vice versâ*. Fever has frequently prevailed over an extensive area, and though cholera, also embracing a large area, approached the other, yet, while the fever continued, epidemic cholera, as an epidemic, has never penetrated the fever field. It appears as if the conditions which generate fever epidemics are incompatible with those which generate epidemics of cholera. Sometimes the one set of conditions, sometimes the other, exists over a large area of the earth's surface, and the one will give way to the other without any marked change in the habits or circumstances of the population those areas embrace. (Mr. R. Lawson, p. 50.)

SCARLATINA.—*Etiology.*—It appears not improbable that blood decomposing under certain states is a factor in the production of scarlatina, *de novo*. When so produced, it, of course, spreads by infection. A number of instances are given in which the disease appeared to commence from the exposure of offal, decomposing blood, or washings from slaughter-houses left to decompose in the open air. In one case there was an epidemic in a village, the public-well of which was proved by analysis to be contaminated with matter from a neighbouring slaughter-house. It appears to be accepted as a determined fact that bad drainage, although everything in the production of typhoid, has no effect in the production of scarlet fever. (Dr. A. Carpenter, p. 1.)

SMALL-POX.—*Effects of Vaccination in lessening the Mortality from.*—Out of 800 cases of small-pox admitted to the Hampstead Small-pox Hospital during rather less than three months, 591 had been vaccinated, of these 58 died, being a percentage of 9·8. 209 were unvaccinated, of whom 96 died, being a percentage of 45·8. The difference is perceptible enough. (Dr. Grieve, p. 46.)

TYPHOID FEVER.—*An unsuspected source of Typhoid.*—A very singular source of propagation of typhoid is described by Dr. Ballard, of Islington. An epidemic of fever broke out in a locality in Islington, and it was noticed that it was confined to the customers of a certain dairyman. The water used by him (of course, only for washing his cans) was found to be contaminated with drainage matter. Dr. Ballard's report of his investigation is extremely interesting. (p. 22.)

*Sulphurous Acid in Typhoid Fever.*—If sulphurous acid is given perseveringly for a week or ten days in doses of from two and a-half to twenty minims according to age, every four hours, the system becomes saturated with it, and the further development of the fever poison is arrested. The patient will complain of tasting or smelling sulphur, and infants will emit the odour of the gas from their skin and breath. In one summer, over 170 cases were thus treated, and only one died—an habitual drunkard. In only one case did diarrhœa set in during the sulphurous acid treatment without having previously existed, but this patient had old standing renal disease. In some of the early cases the acid was left off after a few days use, because the patient seemed better. In almost all such cases they had a relapse, which was again immediately arrested by the resumption of the acid. (Dr. G. Wilks, p. 37.)

ZYMOTIC DISEASES.—*Sulpho-Carbolates.*—Dr. Sansom continues to use the sulpho-carbolates in the zymotic diseases. He finds them capable out of the body of destroying the vitality of the germs of disease, and believes them to have the same property within the body. He gives the mode of preparation of sulpho-carbolic acid, which forms a series of stable salts, with most of the alkalies, alkaline earths, and metals. They have not yet received the attention at the hands of the profession which they deserve. (Dr. A. E. Sansom, p. 11.)

The sulpho-carbolates are extremely soluble, and are administered with the greatest ease, a drachm of sulpho-carbolate of soda (equivalent to about twenty grains of carbolic acid) has been given to an adult patient every four hours. To a child seven years of age ten grains every four hours is the proper dose. (Dr. Sansom, Obs. Trans. p. 6.)

## AFFECTIONS OF THE NERVOUS SYSTEM.

CASES OF SEVERE ABDOMINAL NEURALGIA.—Cases of severe abdominal neuralgia occur not unfrequently amongst the poorer classes, and much resemble peritonitis. The pain is generally acute, the tongue moist, the abdomen very tender on pressure, not tympanitic, although this is sometimes the case. The temperature is generally only a little higher than natural, but a high temperature is occasionally present. In three cases the pulse was 78, 102, and 80 respectively. It is very important that these cases should be recognised. The previous history of the patient is generally of service in the diagnosis, as there is always some cause of exhaustion; also the posture, for the patient will lie with her legs extended, or will double herself up, neither of which positions are seen in peritonitis. Soothing warm applications externally, and opiates internally are generally successful in relieving the pain, and repose and good nourishment in completing the cure. (Dr. H. Jones, p. 83.)

CHOREA.—*Chloral.*—Chloral is the most prompt and efficacious remedy in intense chorea, when the life of the patient is threatened. (M. Bouchut, p. 334.)

*Ether Spray to the Spine.*—Apply the anæsthetic ether spray along the spine for four or five minutes at a time. It produces a strong impression on the nervous system, interrupting the series of morbid actions going forward. In a case treated in this manner successfully, fifteen sittings were required. (Dr. J. Rose, p. 89.)

DEGENERATION OF NERVE STRUCTURE FROM INJURY—(*bearing on Railway Accidents.*)—The author shows that, if the posterior root of one of the spinal nerves is divided between the ganglion and the cord, the root degenerates, and that after a time its continuation upwards in the cord also degenerates. If the same thing is done on the anterior columns, no degeneration in this direction takes place. The law is, that the degeneration takes place in the direction of the physiological activity. If a sensory nerve is injured by an accident, degeneration is liable to take place centripetally, until the ganglion on the posterior root becomes destroyed, the degeneration then proceeds up the cord to the brain. A case of this kind is related where a finger was crushed, and the case ended in loss of vision and speech, paralysis, and death eighteen months after the accident. These progressive degenerations generally extend over several years. Not only injury, but excessive activity, or exhausting use of the sensory nerves and ganglia will affect the nutrition of the sensory fibrils. (Dr. T. Laycock, p. 68.)

LIGHTNING-STROKE.—In lightning-stroke there is liberation of gases from the blood; distension of veins in all parts; pressure on the brain in the closed cavity of the skull, and insensibility as the result of the pressure. The opening of a vein at once relieves all this embarrassment, and the distended right side of the heart is set at liberty to re-commence its contractions, the nervous centres are relieved, and if the blood is not actually disorganised, nor organic structure ruptured, the natural functions are restored, and life is saved. (Dr. B. W. Richardson, p. 91.)

MANIACAL EXCITEMENT.—*Bromide of Potassium and Cannabis Indica.*—Bromide of potassium and cannabis indica, given together, have a most powerful effect in controlling maniacal excitement. The combination has an essentially different action from that of either of them given alone. Immense and almost dangerous quantities of bromide of potassium alone are required to produce an equivalent effect, and there is this disadvantage, that the effects are cumulative and increase for days after the medicine has been stopped, almost paralysing the cerebrum and sympathetic. The usual dose of the combined drugs is one drachm of the bromide and one fluid drachm of the tincture. Opium is not suitable in these cases, for it requires large doses to produce the desired effect, and when it is necessary to continue it for some time it soon ceases to act as at first. This is not the case with the combination recommended. (Dr. T. S. Clouston, p. 79.)

SEA-SICKNESS.—*Chloral.*—If it is wished to avoid sea-sickness in short sea passages it is only necessary to take an ordinary dose of chloral, and sleep during the voyage. Chloral is of great value, however, even during prolonged sea voyages, giving a good night's rest, arresting violent sickness when it has set in, and stopping the tendency to its recurrence. (p. 375.)

SUNSTROKE.—In cases of severe sunstroke as observed in England, with livid face, and prominent veins, there is no remedy like the old one, venesection. The veins, standing out from the extreme blood pressure within, almost offer themselves to the operator. Probably many a case now fatal would be saved if the practice were more commonly adopted. (Dr. B. W. Richardson, p. 92.)

TETANUS NEONATORUM.—Chloral hydrate in doses of one to two grains at the time of each onset of convulsions is the most successful plan of treating this disease. Six cases out of ten or twelve recovered under this treatment, all the others died. (Dr. Widerhofer, p. 78.)

## DISEASES OF THE CIRCULATORY SYSTEM.

ANEURISM.—*Atheroma as a Cause of.*—It would be going too far perhaps to discard the influence of atheromatous degeneration altogether, but certainly, aneurism as met with amongst soldiers frequently exists quite independent of that form of disease of the inner coats of the arteries; and destruction of large portions of even the whole three coats may take place by an acute process, and without a trace of atheroma in the neighbourhood. (Inspector-General Lawson, p. 107.)

CARDIAC CONGESTION.—There are cases of cardiac congestion, with enfeebled right side of the heart, in which under certain conditions there is sudden embarrassment of the circulation, oppression of breathing, tenseness of veins, and threatened death. The body is dusky, the eyes injected, the passive oppression intolerable. The removal of a few ounces of blood will give inexpressible relief, and so far from "reducing" it seems to act quite the other way. (Dr. B. W. Richardson, p. 95.)

NÆVI.—*Removal by the Ecraseur.*—The removal of large nævi may be effected with ease by means of the écraseur. Two hare-lip pins must first be passed through the base of the tumour, and the chain of the écraseur be passed below them. It is well to make a very shallow incision through the skin (to the depth of $\frac{1}{4}$ of an inch), as it is not easily divided by the chain. The chief advantages are the linear cicatrix which results, and the small amount of hemorrhage. (Mr. J. F. West, p. 266.)

*Injection of Perchloride of Iron.*—Injection of perchloride of iron by means of a fine syringe is undoubtedly one of the best means of curing large nævi. If the instrument invented by Dr. Brandt, of Oporto, be employed, all danger of death from embolism is avoided. It consists of two blades on the principle of the Entropion forceps, one of which is to be passed *behind* the tumour (supposing it to be situated in a part in which it can be so placed), whilst the other, which is fenestrated, passes over or around it. The tumour bulges, of course, through the fenestrum, when the blades are brought together by means of a few turns of the screw in the handles. The perchloride may now be injected with perfect safety, and if the tumour is large the process may be repeated as often as necessary. This plan is peculiarly suitable for nævus on the lips and cheeks. (Dr. Brandt, Practitioner, Dec., p. 348.)

TORSION OF ARTERIES.—The author has now systematically for four years twisted every artery in his operations, including nine cases of amputation of the thigh; and without a single case of secondary hemorrhage. His mode of procedure is as

follows. He uses forceps like the ordinary torsion forceps, but the blades are broader and stronger than those usually seen, and terminate in broad well-rounded points. Each vessel is seized and rapidly twisted some six or seven times, the wrist turning with the demi-circuit some twelve or thirteen times, until in fact the operator feels something "give." The safe completion of the torsion is tested by the operator seeing the twisted bit of tissue pulsating away in the face of the flap. (Mr. C. Forster, p. 177.)

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## AFFECTIONS OF THE RESPIRATORY SYSTEM.

GREY AND YELLOW TUBERCLE.—Grey and yellow tubercles are not distinct species of tubercle, but merely stages or varieties of the same thing. The real element in the question is the element of intensity. A tubercle not very intense is grey at first, and by age becomes yellow, but it may always remain grey. A tuberculous deposit very intense in its nature from the first is yellow, there is no previous grey stage. It is an analogous thing to the production of lymph or pus, by a variation in the intensity of the cause. The yellowness which occurs in grey tubercle by age is a fatty degeneration taking place slowly in the original elongated and fibre-shaped elements. The yellow tubercle from the first is composed of round cells with no plastic tendency to elongation, and very early becomes fatty. All tubercles are inflammatory in their origin, just as much so as a herpes or psoriasis; and in cases of pneumonic phthisis we find tubercles graduating into patches of diffused pneumonia. The pneumonia is here, as always, intensely specific, being as much part of the disease as the tubercle, and there is in fact every gradation from tubercles to large patches of tuberculous pneumonia. (Dr. W. Moxon, p. 118.)

CROUP AND DIPHTHERIA.—*Tracheotomy in the later Stages.*—Both these diseases may exist in one of two types, the sthenic or asthenic. It is in the sthenic form alone, where the tendency to death is from suffocation rather than exhaustion, that the operation of tracheotomy is admissible. In many of these cases it is possible to prevent immediate death, and so give longer time for the patient to live through the disease, and ultimately throw it off. In the operation the great maxim is, operate leisurely and without hurry. (Dr. A. Buchanan, p. 126.)

*Glycerine Inhalations in Croup.*—Inhalation of pure glycerine in spray, produced by means of Siegle's apparatus, is being used by some physicians in Mannheim. It is found that the

cough becomes free and moist, and the expectoration easy. It is not of much use in advanced cases. (Dr. G. Stehberger, p. 379.)

IRRITABLE COUGH OF BRONCHITIS AND PHTHISIS.—*Chloral.*—Chloral exercises a decided control over the irritable cough of bronchitis and phthisis. In many of these cases opium is highly objectionable; conium useful, but uncertain; and hydrocyanic acid next to worthless unless given in dangerous doses. Chloral quickly gives rest, and renders the harassing cough less frequent and harsh. (Dr. R. U. Ronayne, p. 333.)

PNEUMONIA.—*Carbonate of Ammonia.*—The use of carbonate of ammonia in cases of pneumonitis should not be confined to the later stages of the disease. It should be given from the very commencement in doses of from five to ten grains every two hours, and it will be found that instead of increasing the febrile excitement and heat of surface, both will be greatly reduced in a short time. The pulse becomes less frequent but full and strong, the skin moist, and the temperature reduced. Out of 96 severe cases so treated only two died. (Dr. A. Patton, p. 129.)

*Arnica as an Internal Remedy in Acute Pneumonia.*—Arnica given internally has a powerful controlling power over the action of the heart and the general circulation. This is particularly seen if administered in cases of surgical fever. In pneumonia, ten minims of the tincture of the British Pharmacopœia every three hours will, in an unmistakeable way, control the severity of the symptoms within forty-eight hours. If the remedy is too long continued the pulse will fall to even 40, and in one case it remained at 40 for several days after the medicine had been discontinued, for its effects are very persistent. (Mr. C. C. Balding, p. 124.)

## AFFECTIONS OF THE DIGESTIVE SYSTEM.

ABSCESS IN CLOSE PROXIMITY TO THE RECTUM.—If there is an abscess in close proximity to the rectum it will soon establish a communication both with the gut and the external parts. Instead, therefore, of opening it in the ordinary way by direct puncture through the skin, it is advisable to adopt the plan first recommended by the late Sir B. Brodie, and pass the knife, guided by the finger, into the rectum, cut outwards into the abscess through the sphincter at once, thus making one operation answer instead of two. For this purpose Brodie's knife is the most suitable, it is shaped like a bistoury, but the outside edge is sharp instead of the inside. (Mr. J. C. Forster, p. 184.)

ACCIDENTAL SWALLOWING OF ARTIFICIAL TEETH.—If a plate with artificial teeth is swallowed, and it is found impossible to extract it owing to its being so low in the œsophagus, or fixed by the hooks of gold catching in mucous membrane, the best plan is to boldly push it down into the stomach. There are so many instances on record where such ugly bodies have passed through the intestinal tract with safety, that the surgeon is quite justified in resorting to this expedient. (Mr. H. Smith, p. 186.)

ACTION OF MERCURY ON THE LIVER.—The author by carefully conducted experiments has determined that mercury has no effect whatever in stimulating the nerves of the orifice of the common bile duct, and so exciting the liver to increased secretion of bile. This supplements the experiments of the Committee of the British Medical Association, and proving that mercury has no action upon the liver either directly or indirectly. (Dr. J. H. Bennett, p. 134.)

HERNIA.—*New Operation for the Radical Cure.*—This operation consists in tying together the pillars of the internal ring by salmon-gut ligature. The ligature is cut off short, and becomes deeply embedded in the minute wound. It does not require removal. The mode of application of the ligature is described. (Dr. A. V. Best, p. 179.)

*Invagination of the Sac—a New Mode of Radically curing Hernia.*—A case of strangulated femoral hernia is related, in which, after the contents of the sac had been returned, by operation, it occurred to the operator that if the sac were detached from its surroundings and invaginated, and retained in this inverted position, the raw surfaces would form adhesions, the opening be thus plugged up, and a radical cure be effected. This was carried out, and the inverted sac carried well within the cavity of the abdomen by the finger. In doing this, the opening of the crural ring, and the edge of Poupart's ligament could be easily felt by the finger covered with the sac. The external wound was closed and compresses of linen placed over it. A perfect cure resulted. Twelve months after the operation, the report is—"the parts had become quite consolidated, and there was no hernial protrusion." (Mr. S. Wood, p. 181.)

NASAL POLYPI.—*The Wire Ecraseur.*—There is no better plan than that of the use of Maw's wire écraseur for nasal polypi. In a case related by the author, at Shanagolden, Ireland, the polypus was so tough, as to actually break a coil of three wires at first used. The loop of wire is to be drawn into the nose by a ligature passed by means of a gum elastic catheter, and for the sake of safety it is as well to pass two ligatures

so that in the event of the wire of the écraseur breaking, a second wire can be passed without delay. (Dr. T. Hayes, p. 187.)

PILES.—Transfix the base of the pile (after the patient has extruded it by pressing over hot water) and then, when he is under chloroform, a free incision must be made on the skin side encircling all the tissues to be removed, and the chain of the écraseur passed over the pin, and into the groove so formed. The operation is complete in a few moments, and a small wound is left which rarely bleeds. The bowels should be kept at rest for two days, and then a dose of oil given. (Dr. G. H. B. Macleod, p. 185.)

TOOTHACHE.—Let the patient apply one or two grains of acetate of lead to the cavity of the tooth, and then spit out any superfluous amount of the salt; it often gives instantaneous relief. (Dr. H. T. Reynolds, p. 379.)

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## AFFECTIONS OF THE URINARY ORGANS.

CHRONIC RETENTION OF URINE.—*Mr. Holt's Winged Catheters.*—In cases of chronic retention of urine, from disease of the bladder, prostate, or urethra, it is absolutely necessary that the bladder should be kept empty, or relieved of its contents when necessary. For this purpose nothing answers like Holt's winged india-rubber catheters. They may be left in the bladder without any inconvenience or distress to the patient, and of course without any tapes or plaster to retain them. It is only necessary to remove them every four or five days to ascertain that no deposit of phosphates is occurring at the end in the bladder. Supposing this is not done, the retained urine produces chronic inflammation of the bladder, and consequent decomposition of the urine, with profuse secretion of mucus and pus. Subsequently, disease of the kidneys ensues. A highly polished silver cap at the end of the catheter prevents the accumulation of phosphatic deposit. These caps are being made by Mr. Baker. (Mr. B. Holt, p. 204.)

*Sir Henry Thompson's India-rubber Catheter for Retention in the Bladder.*—Pass into the catheter a piece of German silver tube four or five inches long, so that the last five or six inches of catheter remains as flexible as ever, also about two inches of the anterior part; then tie round the catheter a stout piece of silk rather more than two inches from the end, which should be furnished with a plug. The silk does not diminish the calibre of the catheter owing to the tube internally, and the tube also renders the instrument easier to pass. To

fasten the catheter in, it is only necessary to tie the silk lightly round the glans penis. (Sir H. Thompson, p. 209.)

DIABETES.—*Organic Changes in the Brain.*—By following the method of preparing specimens of nerve structure for microscopic observation, recommended by Dr. Lockhart Clarke, it is found that there are certain definite changes occurring, especially in the medulla oblongata, in cases of diabetes. These changes are in all probability the cause of the disease, or, more correctly, they are the disease, the saccharine urine being only a symptom. These changes originate in alterations in the vessels of the part, and consist of, in succession—dilatation of the minute vessels with extravasation of their contents; degeneration of the nervous matter round the vessels at certain points; and the production of cavities in the situation of previous nerve decay. Similar changes may occur in other parts of the brain, quite independently of diabetes. (Dr. W. H. Dickinson, p. 135.)

*Skim-Milk Treatment of Diabetes.*—The treatment of diabetes by a purely skim-milk diet offers not merely an amelioration of the symptoms, as does that of a meat diet, but gives every probability of an absolute and permanent cure. Two most interesting cases are related illustrative of the mode of treatment; both were bad cases, with urine of high specific gravity, thirst, and hot dry skin; both were permanently cured, not a trace of sugar remaining in the urine, and all the constitutional symptoms gone. The diet is to be six or seven pints of milk from which the cream has been removed, and no other food must be taken, and no stimulants. The sugar totally disappeared from the urine in a fortnight in both cases, still the milk diet was persevered in for five weeks altogether, when part of the milk was given curded with rennet. At the end of seven weeks a meat dinner, with green vegetables, was allowed daily. The last report of each case is at a period of six months in one, and seven in the other, from the commencement of the treatment. The diet was then as follows:—For breakfast: $\frac{1}{2}$ lb. of mutton chop, a pint of milk, and about $\frac{1}{2}$ pint of coffee. For lunch: $\frac{1}{2}$ lb. of potted head or potted meat and a pint of milk. For dinner: about $\frac{3}{4}$ lb. of roast beef or mutton, chop or steak, fowl or turkey, with green vegetables (brussels sprouts, cabbage, &c.) After dinner, up to bedtime, tea and a liberal quantity of milk are taken. Six pints of milk are consumed daily; from this the greater portion of the cream has been separated. This dietary to be continued for some time longer until it be considered safe to introduce articles of diet containing starch or sugar. (Dr. A. S. Donkin, p. 390.)

DIURETICS.—*The Action of.*—Citrate and acetate of potash and spirit of nitric ether slightly increase the amount of water excreted by the kidneys, but decidedly reduce the quantity both of the urea and solids. Oil of juniper, on the other hand, slightly reduces the amount of water, and increases appreciably both the urea and solids. (Dr. F. B. Nunneley, p. 148.)

INCONTINENCE OF URINE.—Syrup of iodide of iron, in doses of twenty-five minims after each meal, will generally cure this complaint rapidly. Tincture of iron does not act in this manner. (Dr. J. Barclay, p. 141.)

*Chloral.* — Fifteen grains of chloral at bedtime, for a child, answers admirably. It should be continued every night until the habit is quite broken. (Dr. W. Thomson, p. 211.)

*Nocturnal Incontinence of Urine and Semen.*—In many cases both of nocturnal incontinence of semen and of urine there is reason to believe that spasm is an important factor. There is a spasmodic contraction of the detrusor urinæ muscle in nocturnal incontinence of urine. It will be found that fifteen grains of chloral hydrate given every night rapidly cures either of these affections. One dose has been known to permanently cure a case of incontinence of urine, of course, by breaking the habit. Chloral has many advantages over belladonna. The latter frequently takes weeks to produce any marked control over the disease, whereas the action of chloral is immediate, the malady often disappearing after the first dose of the remedy. Moreover, the injurious effect of belladonna upon the eyes is avoided. (Dr. J. B. Bradbury, p. 138.)

LITHOTRITY.—*To Extract the Detritus by Syphon-suction.*—It is very important to remove as far as possible the detritus produced by crushing the stone, with as little injury to the delicate urethral mucous membrane as possible. The simplest and most effectual method of doing this is by syphon power. This is infinitely preferable to direct suction, as it is quite possible to injure the vesical mucous membrane by the latter. The experiment may easily be made with a tumbler of water containing some sand, and a catheter having a caoutchouc tube attached. It will be found that for the withdrawal of the sand, contact with the eye of the catheter is necessary. For use in lithotrity a catheter should be used, short anteriorly, and having no lateral eye, only a terminal opening which is closed during introduction of the instrument by an obturator, for the purpose of preventing injury to the mucous membrane of the urethra. (Prof. Dittel, p. 200.)

PARALYSIS OF THE BLADDER FROM RETENTION OF URINE.—If after a time the bladder does not regain its power, although the water is drawn off regularly, inject it with a weak solution of tincture of iron. The injection should be allowed to remain half a minute and repeated as necessary. (Mr. C. H. W. Parkinson, p. 147.)

STRICTURE OF THE URETHRA.—There is no plan of treatment of universal applicability. The following are the best principles of guidance. First, that all cases in which it is possible to introduce a bougie, should be treated by gradual dilatation, as, in skilful hands, it is the safest, and by far the most effectual, and is the least liable to be followed by a relapse. Secondly, that strictures which are impermeable to catheters should be treated by internal urethrotomy; and thirdly, that strictures which are impermeable to any catheters or bougies should be treated by external urethrotomy. (Dr. W. Stokes, jun., p. 190.)

*Maisonneuve's Method of Performing Internal Urethrotomy.*—The first step is to pass a very fine and delicate filiform gum-elastic bougie. Next, a grooved steel director is attached by its point to the end of the bougie, by means of a screw, and passed into the bladder, pushing the bougie before it. The bougie coils up in the bladder. A triangular-shaped urethrotome is then passed along the director, and the stricture divided. The peculiarity of the urethrotome is the chief thing worthy of note in the operation. The projecting angle of the triangular-shaped urethrotome is blunt, so that its contact with the urethra cannot be productive of any wound or injury whatsoever. The part, however, between the angle and the extremity is sharp, but does not wound the normal part of the urethra, as this is borne off by the blunted projecting angle. This cutting edge comes into contact with the stricture and divides it. When the stricture is divided the instruments are withdrawn, and a large-size vulcanized india-rubber catheter introduced. (Dr. W. Stokes, jun., p. 195.)

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## AMPUTATIONS, DISLOCATIONS, FRACTURES, ETC.

AMPUTATIONS.—*New Method of uniting Flaps by Deep Suture.*—In amputations the deeper surfaces of the flaps should be brought accurately in apposition by means of carbolised catgut ligatures. A large ligature should be passed just underneath the skin, across one flap from right to left, then introduced into the other flap, and passed from left to right, the ends should then be drawn upon until the flaps are

brought into accurate contact, and knotted and cut off short. Of course all the precautions of Lister's system of dressing are used. The external wound is best brought together with a continuous suture of very fine iron wire, made to include the skin only. The knotted end of the deep circular suture is to be completely included and shut out of sight. Before the external suture is quite secured, a syringe is insinuated between the flaps at the spot which remains open, and a jet of carbolised fluid injected to ascertain whether there remains sufficient egress for any fluid that may be poured out from the wounded surfaces. The continuous wire suture will work its own way out, and does not require removal. (Mr. Couper, of the London Hospital, p. 163.)

AMPUTATION AT THE KNEE-JOINT.—The advantages of amputation at the knee-joint are very great, but the surgical mechanists object to it, because the stump is inconveniently long for the subsequent adaptation of a mechanical appliance, the knee joint being thrown lower down in the limb than the natural one, and one thigh consequently appearing longer than the other. This may be obviated by making the femoral section from half to three-quarters of an inch above the antero-superior edge of the condyloid cartilage. This section does not open the medullary canal, and the surface of bone exposed is quite broad enough to bear the weight of the body. In all amputations through the shaft of the femur the weight of the body is supported by the tuberosity of the ischium. (Dr. W. Stokes, jun., p. 149.)

Amputation through the knee-joint offers this great advantage, viz., that the patient can bear the weight of the body without pain upon the extremity of the stump. This is on account of the breadth of the surface of bone. Again, there is little or no risk of pyæmia as when the medullary cavities of the shaft of the femur is opened. If there is no disease of the joint the articular surfaces of the femur and patella should not be interfered with. If, however, there is disease of the joint with ulceration of cartilages, the condyles of the femur should be removed and also the articular surface of the patella or the whole of the bone. (Mr. G. Pollock, p 154.)

AMPUTATION OF THE FOREARM.—*Modified Circular.*—The objection to circular amputation of the forearm are two: first, the difficulty of retracting the soft parts so as to allow of the bones being sawn sufficiently high up; second, the deep hollow left for the retention of pus. The objection to the flap operations is the great risk of the bones projecting at the point of union of the flaps, and this is especially the case

in the lower portion of the limb, where the shape is so flat. By a simple modification of the circular amputation, the disadvantages of both may be avoided, and the advantages of both secured, by making a longitudinal incision on the lower (ulnar) side, from the free edge of the flap to within an inch or so of the point where the bones are to be divided. By this means constant drainage is secured, and the retraction of the flaps is rendered easy. (Dr. G. H. B. Macleod, p. 156.)

ANCHYLOSIS OF THE HIP-JOINT.—*Subcutaneous Division of the Neck of the Femur.*—Previous to the present time only two surgeons (both American) have attempted the relief of anchylosis of the hip-joint by division of the femur. By both, the thigh-bone was divided just below the trochanter major, between it and the trochanter minor, with the object of getting the false joint in the axis of the limb. Neither surgeon operated subcutaneously. Mr. Adams has lately successfully operated in a similar case by subcutaneous division of the neck of the femur within the capsular ligament, dividing the bone near the centre of the neck. A tenotomy knife was first entered a little above the top of the great trochanter, and carried straight down to the neck of the thigh-bone, dividing the capsule freely. A small saw made on purpose was then passed along the track of the knife, and the bone divided from before backwards. It was found necessary to divide some tendons before the leg could be brought straight. The case did perfectly well, and the man was able in the end to bear the whole weight of his body on the limb. (Mr. W. Adams, p. 159.)

ANTISEPTIC SYSTEM OF TREATMENT IN SURGERY.—Professor Lister lays great stress upon the necessity of operating in an antiseptic atmosphere. This is accomplished by an assistant playing upon the wound with a carbolic lotion of 1 in 40, by means of a Richardson's ether spray apparatus. The next step after closing the wound is to cover it with oiled silk "protective," dipped in the lotion, to give it a temporary antiseptic film. Instead of covering this with lac plaster, as hitherto, Mr. Lister now uses oakum, or, what is better still, about eight thicknesses of muslin gauze, dipped in the following mixture whilst melted:—Sixteen parts of paraffin, four parts of resin, and one part of crystallised carbolic acid. The muslin must be pressed or wrung whilst the mixture is hot, to remove the superfluity. In order to prevent the discharge soaking directly through it, a piece of thin gutta-percha tissue may be placed beneath the outer layer, to guide the fluid towards the edge of the cloth. (Prof. Lister, p. 165.)

BLOOD-POISONING.—*Treatment by a Carbolised Atmosphere.*—In cases of erysipetalous and pyæmic blood-poisoning, and also as a prophylactic agent after operations, the vapour of carbolic acid, if evolved constantly under and retained by the bed-clothes, is absorbed into the system, as in a vapour-bath or fumigation. By this plan, not only is the system affected, but the wound is kept totally free from putrescence or smell of any kind, when large sloughs are not actually present. This may be carried out best by suspending small muslin bags of Macdougall's disinfecting powder from the ribs of the cradle, which is almost universally used to keep off the weight of the bed-clothes, or to swing the limb in surgical cases. By this means we avoid, on the one hand, the local irritation and the retarding action of the acid upon the granulations of the wound or sore, which we leave free for the application of any other stimulating agent that may be deemed advisable, and for the regular and effective cleansing, which is so powerful an aid to healthy action. (Mr. J. Wood, p. 49.)

DRESSING FOR WOUNDS.—*Picked Oakum.*—What is wanted is a dressing non-irritating, very absorbent, capable of retaining lotions and of being carbolised, and, for general, and especially for hospital use, of an inexpensive character and easily obtainable. All these requirements are found in picked oakum. Amputations may advantageously be treated without dressings of any kind, only they must be placed on a protected pillow, first putting under them a double handful of the oakum, which absorbs the secretions, and prevents them running off the mackintosh on to the bed. There is only one objection to its use, and that is its *apparent* roughness, but as patients do not complain of this, it is more an apparent than a real objection. (Dr. T. H. Bartleet, Lancet, May 13, p. 642.)

EXCISION OF THE KNEE-JOINT.—There is a tendency after excision of the knee-joint to have displacement of the femur unless special precautions are taken to prevent it. There may be projection forwards of the lower end of the femur produced by a constant sinking of the buttock in bed; this is best obviated by the use of a hard unyielding mattress. Again, there may be a projection outwards of the lower end of the femur, accompanied with a certain amount of rotation —this proceeds from muscular action, and is best prevented by the use of a very long side splint, extending from the foot to the axilla. If extended to the axilla we may counteract a tendency to an angular twist of the body to the opposite side in bed, whereby the lower end of the thigh is abducted or

everted, with an angular projection outwards at the knee. It is well to have the foot piece attached to the side splint in order that all pressure on the heel may be avoided. (Mr. F. J. Gant, Lancet, May 13, p. 640.)

EXHAUSTING NEEDLE-TROCAR.—*Dr. Protheroe Smith's.*—This is an instrument of great value. It consists of an ordinary glass syringe, having two tubes at its distal extremity at right angles to the body of the syringe, and so arranged as to valves that when the piston is raised fluid is imbibed by one, and when it is depressed it is expelled by the other. Pieces of elastic tubing, are placed on both tubes of the syringe—that on the entrance tube being of gum-elastic, terminating in the exploring needle. The exploring needles are of various lengths and thicknesses. Two or three should be very long and fine. They are made of steel, gilt. The instrument can be used wherever acupuncture can be employed, for the purpose of diagnosing, evacuating, and treating tumours and effusions of all kinds, whether of joints or cysts containing viscid fluid or otherwise, whether declared by fluctuation or only by the history of the case pointing to this probable issue; in abscess or in abnormal effusion in any of the serous cavities, and in extreme cases of retention of urine, without injury to the bladder. It is also equally efficacious in disengaging excessive collections of flatus, which often prove so distressing in tempanitic distensions of the intestines as to defy all other modes of relief. In the same way, without removing the instrument, remedies can be injected into cysts, abscesses, &c., without any risk of the admission of air. (Dr. P. Smith, p. 173.)

FRACTURES OF THE LEG.—*Use of Glue-Bandages.*—What is called the glue-bandage is made by impregnating a roller with a solution of French glue in methylated spirit. When applied a sufficient number of times to the limb to make it firm, it is cut up in front, and finally laced through eyelets put into the cut edges. It is very light and elastic, and can be made of any degree of strength. A firmer mould may be made by putting adhesive plaster within and without the glued portion, and a thin layer of cotton wool being put next the leg. (Dr. G. H. B. Macleod, p. 177.)

*Simple Fracture of the Leg in Children.*—Apply adhesive plaster in a succession of stripes from the foot upwards (of course, after adjustment) and then two light pasteboard splints, retained at their upper and lower ends by a narrow circular band of plaster. (Dr. G. H. B. Macleod, p. 177.)

## AFFECTIONS OF THE SKIN, ETC.

ACUTE GENERAL PSORIASIS.—Depending as this disease does upon active congestion of the skin, no remedies should be employed unless extremely soothing ones. Alkaline baths with subsequent oiling of the skin, combined with cod-liver oil and good living will generally soon cure cases of this kind, in children especially. In many cases tarry applications irritate, and consequently prolong the case. (Dr. T. Fox, p. 245.)

CARBUNCLE.—*To Arrest.*—The first appearance noticeable when a carbuncle is about to form, is a small vesicle containing matter. This is the virus which is the real cause of the subsequent inflammation, and if this virus is destroyed the carbuncle may be nipped in the bud. For this purpose touch the vesicle for a fraction of a second with an incandescent lucifer match, or red-hot wire, from five to seven or eight times in succession, when it assumes a dull whitish appearance from coagulation of the albumen it contains. The pain of the operation is really trifling, and it will save from a week to a fortnight's suffering. (Dr. W. Marcet, p. 273.)

*Potassa Fusa.*—However it acts, whether by modifying nutrition so that inflammation subsides and the cores are reabsorbed, it is difficult to say, but if a carbuncle be scored crucially with potassa fusa, so as to make a superficial scar, the inflammation does subside rapidly, and without suppuration if it is done sufficiently early. The core is not sloughing cellular tissue, but a fibrinous coagulum, the result of inflammation. (Dr. J. Murray, p. 269.)

ECZEMA.—It is a good plan to combine with the benzoate of zinc ointment a small quantity of spirits of wine, in the proportion of one drachm to the ounce; the spirit softens the ointment and facilitates its application; it produces a feeling of coolness which is agreeable to the heated surface, and it has besides a gentle stimulant effect on the nerves of the skin. The application relieves the heat, the stiffness, and the itching, and the relief continues until the ointment dries up or is accidentally removed. (Mr. E. Wilson, p. 259.)

*Eczema in Children.*—Firstly, we should cover every visible part of the eruption, whatever its state, and avoiding only the hairy scalp, with the benzoated zinc ointment in combination with spirits of wine; secondly, we should examine carefully into the diet, and direct such a regimen as in our opinion is most likely to be nutritive; and, thirdly, we should administer from one to two minims of Fowler's solution in combination with iron three times a day, after meals. It is

well to see that the bowels act regularly. There is no medicine more certain in its effects, more harmless, and more successful than arsenic. The dose for an infant of a month or six weeks old, may be one minim of Fowler's solution, equal to the $\frac{1}{120}$ of a grain of arsenious acid, a dose far too minute to do harm, although capable of doing wonders in the way of good. (Mr. E. Wilson, p. 264.)

*The proper Use of Soap in Eczema.*—In the early stages of eczema washing the skin is to be avoided, it would irritate and cause disturbance where rest and soothing measures are required. But in the later stages of the disease when the skin becomes thickened and indurated, and the eruption inveterate and lethargic, we require some local stimulant or tissue tonic, the first and best of which is undoubtedly soap. A chronic eczema should be thoroughly washed with soap, a certain amount of friction and compression being at the same time used. After the washing it is to be dried with a soft napkin and dressed with zinc ointment, precisely like an acute eczema. The washing should be repeated daily unless we see signs that the irritation produced is excessive. The stimulant treatment causes an exudation on the surface, which relieves the infiltration of the tissues within. (Mr. E. Wilson, p. 262.)

ERYTHEMA OF THE FACE AND ACNE.—In cases of this nature, when acute, and in the early stages, soothing remedies are required. None answers better than a lotion made with half-an-ounce of pale prepared calamine powder, frequently applied after hot bathing, with suitable internal remedies to meet dyspepsia, pyrosis, and uterine troubles. All irritating applications do harm, such as the soap treatment in vogue at Vienna. (Dr. T. Fox., p. 247.)

IN-GROWING TOE-NAIL.—Cut out a triangular portion of the centre of the nail, having a wide base at the free edge of the nail and a fine point at or near the matrix. This will cause the nail to contract from the edges towards the centre, and if kept up for six months will quite alter the shape of the nail, making it filbert-shaped, and prominent in the centre. The edges of the nail should be raised and separated from the soft parts imto which they intrude with a piece of worsted coated with mercurial ointment. When exquisitely sensitive granulations exist some extract of belladonna and resin ointment rubbed together forms a good application. (Dr. J. Waring-Curran, p. 175.)

LICHEN RUBER.—This is a very rare disease in England. It does however occur. It is characterised essentially by the development of solid *red papules*, caused by effusion of lymph about

the hair follicles. It is dependent upon disorder of the sympathetic nervous system. It is very easy to aggravate the congestion. Too hot a bath will do it, the heat of the bed, warm drinks, a cold wind; even arsenic, which is said to be *the* remedy for the disease will not unfrequently act in the same way. The most soothing remedies are required. (Dr. T. Fox, p. 247.)

SKIN-GRAFTING.—The process of skin-grafting does not consist in a transplantation of a portion of real skin, it is only cuticle, and, consequently, the growth which results is merely a cicatrix, in every respect identical with that formed spontaneously in the natural process of cure. (Mr. D. Page, p. 257.)

It is most essential to success that the granulating surface is in a healthy condition. A good guide is the tendency to form marginal cicatrisation. Not only will the minutest portions of skin answer the purpose, but larger pieces, probably even as large as a penny, may with success be transplanted. Transplanted skin does not retain its perfect integrity and function as does skin employed in Talicotian operations, since we cut it so close to its under surface, in order to avoid fat and areolar tissue, that we cut through most, if not all, the sweat glands and hair bulbs. If the granulations are not quite bright and active, they should be scratched or slightly incised, and the graft laid on when the bleeding has stopped. It may be bound in its place by a strip of Lister's lac plaster, or a piece of gutta-percha tissue covered by strapping. A compress of cotton-wool should be placed over the strapping, and over all a bandage rather firmly applied. The advantage of the gutta-percha is that, being transparent, it enables us to see, while stretching it across, that the grafts do not slip. (Mr. C. Steel, p. 252.)

*Skin-Grafting for the Cure of Ulcer on the Leg.*—The case of a man who had had an ulcer on the leg for twenty years is related, which had resisted all means of cure. Three pieces of skin, the size of a pea, were removed from the forearm, and placed on the surface of the wound. Some pains had previously been taken to get the surface of the sore as healthy as possible. These were bound to the granulating surface with ordinary soap plaster. In three days the plaster was removed, and the morsels of skin no longer looked white, but were of a reddish-blue colour, and elevated in the centre. No further change took place for seven or eight days, when they rapidly commenced to increase in circumference. The ulcer was completely cured. It is better to employ a dressing forceps and bistoury in taking the grafts, instead of the scissors. (Mr. R. W. Goldie, p. 258.)

## VENEREAL AFFECTIONS.

CHORDEE.—The most effectual remedies for chordee are a fifth of a grain of morphia injected under the skin at bedtime, or a suppository of one-third of a grain of morphia and two grains of extract of belladonna. (Dr. B. Hill, Lancet, April 29, p. 570.)

GONORRHŒA.—Arrest of a gonorrhœa must not be attempted after the discharge has begun, but, before that period, that is in the very earliest stage, the disease may often be arrested by the injection, every hour or two hours, of half or quarter of a grain of nitrate of silver, or half-a-grain of sulphate of zinc or tannin, to the ounce of water. (Mr. B. Hill, Lancet, April 29, p. 570.)

*Permanganate of Potash Injections.*—Permanganate of potash, of the strength of five grains to the ounce of water, is a valuable injection in cases of gonorrhœa and gleet. The strength may be increased up to fifteen grains. It should be used four times a day. As the permanganate loses its virtue by admixture with any extraneous matter, or exposure, it is advisable to mix the injection immediately before using it. (Dr. T. Warden, p. 275)

Injections of permanganate of potash, five grains to the ounce, are of great service in the treatment of gonorrhœa, both acute and chronic. This plan of treatment is not, however, so suitable for cases of extremely chronic gleet as the tincture of iron internally. Out of sixteen cases of acute gonorrhœa, ten were cured in six days on an average. (Dr. W. M. Campbell, p. 274.)

*Tannin and Glycerine.*—Tannin mixed with glycerine forms a waxy mass, which soon becomes smooth, hard, and brown, but readily dissolves under a gentle heat. Rods suitable for introduction into the urethra can be made of tannic acid 2 parts, powdered opium 0·12 parts, and a sufficiency of glycerine. A piece about an inch and a-half long is left in the urethra, where it melts sufficiently to be expelled by the urine in five or ten minutes. The remedy is to be applied twice or thrice daily. (Dr. Schuster, p. 273.)

*To Cure Gonorrhœa in One Week.*—Give no internal medicine whatever, and direct an injection of two grains of sulphate of zinc to the ounce of water, to be used at least four times a day, and much oftener if there is the opportunity to do so. The injection should be continued at least a week after the discharge has entirely ceased, and it should not be left off suddenly, but gradually. (Dr. P. Foster, Lancet, May 13, p. 666.)

SYPHILIS.—*Subcutaneous Injections of Corrosive Sublimate and Common Salt.*—Corrosive sublimate and common salt combine chemically, and form a soluble compound which does not precipitate albumen. Two parts of sublimate, and from 20 to 26 of salt, to 1000 of water, form a suitable solution, which causes no heat or pain, and the use of which is not followed by the formation of abscesses. Syphilitic ulcers heal rapidly under the topical application of this solution:—200 parts of water, 1 part of sublimate, and 10 of common salt. (Dr. Stern, Lancet, May 6, p. 617.)

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## AFFECTIONS OF THE EYE AND EAR.

CATARACT EXTRACTION.—*After-Treatment of.*—After the operation, the eyelids should be closed with a couple of strips of court-plaster, about an inch and a half long and a quarter of an inch wide. Bandages and compresses of all kinds are injurious. They are hurtful in proportion to their action. Moreover, they produce heat of the eye, and soak up the secretions, which ought to have free vent. The practice of opening the eye a few days after the operation cannot be too strongly deprecated. The examination is useless if the eye is doing well, and if otherwise, it is certain to aggravate the evil. The state of the eyelid is an excellent guide to the state of the corneal wound. A red puffy lid shows an inflammatory state of the eyeball, and want of repair in the wound. After seven days, success is pretty certain, if no unfavourable symptoms have occurred, and the plasters may be removed after they have been thoroughly softened with warm water. (Mr. H. Walton, p. 387.)

*Removal of Soft Cataract by Absorption.*—It should be our great principle, in procuring absorption of a soft cataract by admission of the aqueous humour, to avoid all movement of the lens from its natural position. If much force is used in puncturing its capsule, or if the laceration is made too free, the lens is liable to become dislocated, causing chronic mischief in the eye and even disorganisation. It must be remembered that swelling of the cataract always ensues upon admission of the aqueous humour to it, and that thereby the tear in the capsule is often considerably enlarged. It is, therefore, well not to open the capsule too freely. It is preferable to introduce the needle through the cornea rather than through the sclerotic. (Mr. H. Walton, p. 239.)

*Lamellar Cataract.*—It is very important in a practical point of view to recognise this form of cataract, as it is often best treated by the formation of an artificial pupil without any other

operation. It is a central opacity of the lens surrounded by a zone of lens substance, either quite transparent or here and there studded with small opacities. It depends of course upon the width and transparency of this zone whether an artificial pupil will suffice for relief. The cataract consists of a layer of opaque lens substance lying between the transparent nucleus and a clear portion of the cortical substance. (Mr. J. S. Wells, Lancet, April 15, p. 497.)

GLAUCOMA.—In excising a portion of iris for the relief of the tension of glaucoma, it is essential to success that the iris should be carefully and completely removed from the wound, for any inclusion of it in the cicatrix will certainly increase the secretory irritability of the eye. The operator should himself use the scissors for removal of the portion of iris, as, feeling at the same time the tension, he can apply the convexity of the scissors far more accurately to the edges of the wound than can the assistant. (Von Gräefe, p. 237.)

HYSTERICAL PTOSIS.—There is nothing like a smart galvanic shock for curing hysterical paralysis. A girl was brought to the Birmingham Eye Hospital who could not open her right eye. When, however, her attention was withdrawn from it by a pretended examination of the left eye, and the right eye was during the examination opened, it remained open until she recollected and closed it. A strong shock passed from one canthus to the other cured it. This is a parallel case to the more common one of hysterical aphonia, which is curable by electricity directly applied to the vocal cords by Dr. Morrell Mackenzie's apparatus. (Mr. F. H. Hodges, p. 242.)

OBLIQUE ILLUMINATION OF THE EYE.—Oblique illumination of the eye is of great use in ascertaining the condition of the crystalline lens, and even the anterior part of the vitreous humour. The light from a good lamp, placed at one side of the patient and a little in front of him, is to be concentrated upon the eye by means of a strong convex lens of two or three inches focus. Any opacity of the lens or vitreous is at once detected, appearing white upon a black ground. A second lens may be employed as a magnifying glass. (Mr. J. S. Wells, p. 224.)

OBSTRUCTION IN THE EUSTACHIAN TUBE.—These cases, which generally arise from catarrh, are often curable by the following method, known as Politzer's. Place the tube of an india-rubber force pump within one of the nostrils of the patient, and then close the other by pressure with a nasal pad. The patient then swallows by degrees, a small quantity of water previously taken into the mouth, and the Surgeon at each

successive act of deglutition, compresses the air bag held in his own right hand. Air is forced each time up the Eustachian tube, overcoming at once any moderate degree of obstruction. (Dr. P. Allen, p. 244.)

OTORRHŒA.—*Carbolic Acid.*—The use of the following lotion is productive of almost uniformly good results in chronic otorrhœa. Carbolic acid, one drachm; glycerine, one ounce; distilled water, five ounces. (Mr. J. P. Pennefather, p. 243.)

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## MIDWIFERY, ETC.

CHLORAL IN LABOUR.—In a case where the pains are pretty strong, but progress is arrested owing to rigidity of the soft parts, give twelve-grain doses of chloral every hour. The sufferings of the patient are to some extent relieved, and at the same time relaxation of the parts is produced. (Dr. J. C. Du Hamel, p. 386.)

CHRONIC PERFORATING ULCER OF THE BLADDER.—*Formation of an Artificial Vesico-vaginal Fistula.*—In this disease, the pathology of which is described by Rokitansky as a limited perforating ulcer, the pain on distension of the bladder with urine is very great. It is relieved by micturition, but returns again directly the bladder commences to refill. The patient becomes in time completely worn out with suffering. The only plan of successful treatment is to establish an artificial vesico-vaginal fistula so as to give the bladder complete rest. This is done by passing a grooved staff along the urethra and slitting up the posterior fourth of the canal, and about an inch of the posterior wall of the bladder. There is no difficulty in getting the fistula to close after the ulcer has healed; the difficulty is to get it to remain open long enough. (Mr. L. Tait, Sir J. Y. Simpson, p. 318.)

DIPHTHERITIC POISON.—*Absorption of, per Vaginam.*—An outbreak of diphtheria recently occurred in the obstetric wards of Guy's Hospital. In the first case which occurred there was a large diphtheritic patch between the right thigh and swollen labium. In all the other cases which occurred there had been some slight operative interference; for instance, in one a fragile calculus was removed from the urethra, and in a few days the parts which had been abraded were covered by a diphtheritic layer running up to the bladder. Such a case appears to prove that the poison may enter the system by an abraded surface, and when this is the case it usually acts very rapidly. (Dr. J. B. Hicks, p. 130.)

DYSMENORRHŒA.—*Mechanical.*—It is often very difficult to introduce a sea-tangle tent owing to the axis of the cervical

canal not being that of the instrument as it is held by the operator. To obviate this, Mr. Godson, of St. Bartholomew's Hospital, uses a tent holder having a ball-and-socket joint near the extremity, which enables the tent to move in any direction and find its way without violence through the canal of the cervix. (Mr. Godson, Lancet, April 22, p. 535.)

FIBROUS TUMOUR OF UTERUS.—*To Arrest Hemorrhage from.*—Having by means of an ordinary sound ascertained the length and direction of the uterus, pass a hollow one into the organ. A syringe composed of vulcanite containing about a drachm of the liq. ferri perchloridi, is fitted closely into the orifice at the proximal end of the probe, and its contents are gently thrown into the womb. No pain is generally felt as a result of this injection, but a feeling of burning is sometimes complained of. This simple procedure will in most cases arrest the hemorrhage from uterine fibrous tumour. (Dr. J. M. Duncan, p. 289.)

FORCEPS.—*Use of the Long Forceps Early in Labour.*—General practitioners delay much too long before applying the forceps, and especially the long instrument. A more early use of the forceps when the head is above the brim would probably save many lives. (Dr. F. H. Daly, p. 280.)

OVARIOTOMY.—*Mode of Securing the Pedicle.*—The clamp ordinarily used for securing the pedicle by its shape and form hides from view the part of the wound immediately beneath it, and it is difficult to dress it and keep it clean and dry. The escape of the pedicle altogether is also liable to occur. To remove these objections Mr. Graily Hewitt has invented an instrument, consisting of a framework of steel something like a shoe-buckle in shape, two and a half inches long by three-quarters of an inch wide. Three small holes or buttons project on each of the long sides and one on each of the ends. These buttons are all projecting from the same surface of the framework and at right angles to it. The pedicle is perforated by a needle armed with a double strong thread in two or three places, according to its size, and is then tied in segments. The frame is then placed round the end of the pedicle and the ends of the threads secured firmly to the buttons. By this plan the cut end of the pedicle is freely open for inspection and treatment. (Dr. G. Hewitt, p. 291.)

*An Improved Method of Dividing the Pedicle.*—None of the methods of dividing the pedicle in cases of ovariotomy quite answer the ends desired. They either do not completely arrest the hemorrhage, prevent union of the external wound, or there is returned into the abdomen a small portion of devitalised tissue. By firmly seizing the pedicle with one pair of

forceps, and twisting the distal end of it by a second pair of forceps, the pedicle may be twisted off close to the edge of the instrument by which it is held. It is necessary that the torsion should be very slowly performed. The forceps to be employed are made expressly for this purpose. Their construction will be understood at once on reference to the woodcut at p. 296. The arrest of hemorrhage by this plan is most effectual. (Dr. G. H. B. Macleod, p. 293.)

PREGNANCY.—*To Calculate the Day of Confinement.*—The average duration of pregnancy is 278 days, rather shorter than has been generally taught. To calculate the time of delivery the plan is simply as follows: Find the day on which the female ceased to menstruate, or the first day of being what she calls well. Take that day nine months forwards as 275 days, unless February is included, in which case it is taken as 273 days. To this add three days in the former case, or five if February is in the count, to make up the 278. There are, however, so many irregularities that the date arrived at only gives the week during which the confinement will probably take place. (Dr. J. M. Duncan, p. 277.)

PROCIDENTIA UTERI.—*Influence of the Perineum.*—The perineum has no direct effect in maintaining the uterus *in situ*, consequently relaxation or rupture of the perineum does not by any means involve procidentia. In the majority of cases the perineum is either entire or not more injured than it is in the great mass of women who have borne a child. There can, however, be no doubt that laceration or relaxation of the perineum favours the occurrence of procidentia in cases where the causes of this accident are in operation. In the treatment of the disease it is a great mistake to suppose that a perineum, however renewed, removes any cause of its occurrence. The restored perineum may or may not succeed in resisting the progress of the descending uterus. It can only cure when its rigidity is sufficient to effectually oppose the progress of the uterus trying to force its way over it. (Dr. J. M. Duncan, p. 310.)

PUERPERAL CONVULSIONS.—*Chloroform.*—In cases of puerperal convulsions either before or after delivery, we have an agent in chloroform by which we can control the disease almost at pleasure. With this at command it appears doubtful whether it is advisable in the first stage of labour to resort to any mechanical means for dilating the neck of the womb, notwithstanding the perfection with which that proceeding can be accomplished. The success attending the continuous administration of chloroform is really wonderful, providing that in severe cases it is given sufficiently long and perseveringly.

In mild cases it is sufficient to keep the patient very slightly under its influence in the intervals of the fits, more being given when indications of an attack are seen. If the convulsions have already produced much pulmonary congestion it is beneficial to withdraw a few ounces of blood before administering chloroform, and generally it is advisable to lessen the tendency to cerebral congestion by the application of cold to the head. (Dr. J. J. Phillips, p. 285.)

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## MISCELLANEA.

ANÆSTHETICS.—*Nitrous Oxide Gas.*—Anæsthesia from the inhalation of nitrous oxide gas is usually procured in one minute and twenty seconds. In thirty seconds the colour of the skin turns slightly livid, and in sixty seconds the pulse falls in force and frequency, the breathing becoming often laboured and sometimes stertorous. A nervous twitching of the hands is often the most prominent mark of complete anæsthesia. There is never any of the struggling so common during the excited stage of chloroform. Recovery is usually rapid and complete, the patient waking up as if from sleep. The longest period during which the anæsthesia lasts is one minute, but, by alternating it with air, insensibility has been kept up for as long as twenty minutes. Dr. Richardson considers that it acts simply by producing asphyxia, and is, therefore, a most dangerous anæsthesia. It appears likely, however, to supersede chloroform in all operations not lasting over one minute, its advantages being—1, Its safety; 2, The absence of troublesome after-effects; 3, The rapidity of its administration; 4, The sitting position is the most suitable for its administration; 5, The absence of anything unpleasant in its taste and smell. The only fatal case which has occurred fairly from its administration was one of phthisis, so that the existence of that disease should be accepted as a contra-indication of its use. (Dr. Maclaren, p. 337.)

*Chloromethyl (Bichloride of Methylene).*—Chloromethyl is superior to chloroform as an anæsthetic in Surgery. Mr. Spencer Wells has now employed it in 180 cases of ovariotomy, in some 25 other cases of gastrotomy, and in more than 50 operations of more or less severity, such as herniotomy and removal of tumours. In not one of these cases did any unpleasant symptoms occur, although the anæsthesia was in some very prolonged, as much as forty-five minutes to an hour or more. He says, "I have never been at all uneasy in any one of these cases, more than 250 in number, either dur-

ing the administration of the anæsthetic, or from any subsequent ill effects fairly attributable to it. Whereas, with chloroform, I never felt quite at ease." There is much less tendency to sickness after the use of chloromethyl than after that of chloroform. (Mr. T. S. Wells, p. 345.)

*Mode of Administering Chloroform.*—The following is the mode of administering chloroform in use at St. Bartholomew's Hospital. A bit of lint is laid lightly over the patient's face, and the chloroform dropped upon it from a graduated bottle, the stopper of which is perforated and drawn out to a fine point. Every now and then the lint is reversed as more chloroform is dropped out. (Mr. Bloxam, p. 367.)

*Carbolic Acid as a Local Anæsthetic.*—Carbolic acid possesses wonderful powers as a local anæsthetic. If a portion of skin is covered with a cloth soaked in a saturated solution of carbolic acid for half an hour, and then a streak traced across the surface with a camel's-hair brush dipped in acid liquefied by one-twentieth its bulk of water, the skin may be divided along the course of the streak with a sharp scalpel, quite down to the subcutaneous cellular tissue, without causing any pain. Abscesses, whitlows, and buboes may be opened with great advantage in this manner. Sometimes it is necessary, when making an incision through the integument, to brush out the wound made with some liquefied acid before making the incision deeper. (Dr. J. H. Bill, p. 171.)

*Oil of Peppermint as a Local Anæsthetic.*—Oil of peppermint lightly applied to the seat of pain, with a camel's-hair pencil, will often give instantaneous relief in cases of facial neuralgia. It is so used by the natives of China. (Dr. A. Wright, p. 368.)

ARSENIC.—*Therapeutic Action of.*—The action of arsenic is twofold: 1st, Upon the blood; 2nd, Upon the skin. Its action upon the blood arises from its property of direct combination with the blood globules. This combination is made at the expense of the oxygen, of which arsenic takes the place. This change in the globules lessens the tissue changes, or the denutrition of the body. This action is evidenced by the diminution in the amount of urea secreted, and by reduction of the temperature of the body. Its action upon the skin is caused by its power of producing injection of the superficial capillaries, whereby increased nutrition of the epithelial surface takes place. with a rapid growth of epithelium. All the three symptoms—the silvery tongue, the plump appearance of the face, and the red conjunctiva—are indications of this action, and are explained by it. (Dr. J. Cleland, p. 249.)

CHLORAL.—*What Quantity of Chloral Hydrate can be given with safety, in divided doses, during a stated period, say of twenty-four hours?*—The body cannot decompose and throw off the the hydrate more rapidly than at the rate of from five to seven grains per hour. Although there must be variations according to age and other circumstances, they are not such as to alter materially the rate of action from the estimate given. One hundred and twenty grains, administered in divided doses, in twenty-four hours, is the safe limit of administration. (Dr. B. W. Richardson, p. 322.)

There is no doubt that what is considered a moderate dose of chloral, thirty grains, is too large a dose to commence with, especially for a patient in whom the effects of the chloral have not been previously tried. Ten or fifteen grains will often answer the purpose perfectly well. (Dr. H. W. Fuller, p. 326.)

*The Vehicle for Chloral.*—The effect of chloral is modified by the vehicle in which it is administered. If given in syrup it is absorbed slowly, and its effects are more prolonged. This is consequently the best form of vehicle when it is wished to procure a night's sleep. If given in a thinner vehicle, as camphor water or simple water, it shows its effects far more rapidly, and they pass off in a shorter time. So rapidly may chloral be absorbed and decomposed in the blood, that the chloroform is let free with too great rapidity for safety. (Mr. H. F. Smith, p. 328.)

EMBALMING THE DEAD.—Dr. Vivodtsef, a Russian Physician, has invented a method of embalming the dead by means of carbolic acid. It is cheap, easy of execution, and effectual. An alcoholic solution of carbolic acid is injected by means of a suitable apparatus into the principal arteries, so that the whole body becomes thoroughly saturated with the acid. The apparatus (of which an engraving is given) and mode of procedure will be found fully described. (Dr. Vivodtsef, Dr. G. L. Carrick, p. 381.)

GLYCEROLE OF STARCH.—This useful preparation is made by rubbing well together one part of starch in eight of glycerine; the mixture being then gradually heated to 240° Fahr., and constantly stirred till a translucent jelly is formed. As a local remedy in many acute affections of the skin, and to prevent the pitting of small-pox, it deserves a more extensive trial. It is a capital substitute for lard in making ointment. (Dr. H. S. Purdon, p. 371.)

HÆMOSTATIC COTTON WOOL.—Soak the cotton for an hour in a solution containing 4 per cent. of soda, then wash and dry.

Dip it one, two, or three times in a dilute solution of perchloride of iron, dry, and pull apart by the fingers. It is hygroscopic, and an excellent dressing for wounds. (Dr. Ehrle, p. 179.)

HYPODERMIC INJECTION OF MORPHIA.—When morphia comes to be used hypodermically as a habit, and in large doses, it generally arises from a non-appreciation of the difference between a stimulant and a narcotic dose. There *are* cases, but they are exceptional, in which we are obliged to permit the use of narcotic doses, such as in malignant tumours. A stimulant dose of morphia is that dose which relieves pain but stops short of narcotism. This is from $\frac{1}{12}$ to $\frac{1}{4}$ grain, and will procure all we want in nearly every case. Narcosis is a depression of nervous life. The first effect of narcotic doses is slight and "functional" depression, but this depression becomes permanent if the morphia is continued in narcotic doses. A stimulant dose produces neither stupor, contracted pupil, or subsequent constipation of bowels or burning of tongue; a narcotic dose produces all these. (Dr. F. E. Anstie, p. 347.)

INFECTION.—*Ready Method of Preventing.*—There is perhaps no plan of preventing infection so ready as the production of sulphurous acid by the combustion of sulphur. To disinfect a bed, whilst the patient is temporarily removed from it, pass a copper warming pan into the bed, containing a few live embers and a little sulphur. The pan should be moved about during the ignition of the sulphur. By burning sulphur in an open vessel, closets, carriages, passages, and vacated chambers of the sick may be easily disinfected. Clothing may be lightly sponged over or sprinkled with water containing a little well-mingled sulphur, and then ironed with a flat iron heated to a temperature which will cause volatilisation of the sulphur without burning the linen. (Mr. J. Startin, p. 379.)

KALI-KUTKI.—*A new Tonic.*—Kali-kutki root is well known in the Indian market. It is very light and brittle, brownish-white in colour externally, and deep black internally, with short waxy fracture. It is an excellent tonic, equal to gentian and calumba. (Mr. M. C. Cooke, p. 374.)

LEECHES.—*How to Apply.*—Always before applying leeches apply a mustard plaster to the part. The leeches will bite with the greatest avidity, and the flow of blood is far more copious than would be thought likely or possible. So much so is this the case that caution is required, only two or three being applied instead of half a dozen. If bread poultices are applied afterwards the blood flows as if it would never stop. (p. 378.)

SOLUTION OF SANTONINE.—The insolubility of santonine impairs its utility. The only fluid which will dissolve any appreciable quantity is a hot solution of carbonate of soda. It is prepared as follows: Santonine, in powder, 12 grains; carbonate of soda, 20 grains; distilled water, 3 ounces. Put the soda and water into a flask, keep the fluid near the boiling-point, adding, as it disappears, about two grains of the santonine at a time, until the whole is dissolved. Solution is effected in about half an hour, during which time the water is reduced to ʒij. If so reduced, ʒj will contain a full dose —six grains of santonine. If an alkaline reaction be objectionable, neutralize with acetic acid. Both solutions, the alkaline and the neutral, are bright, clear, and permanent. These solutions may be used topically, as in parasitic disease of the bladder. (Dr. J. Harley, p. 371.)

STRYCHNIA POISONING.—*Bromide of Potassium.*—Bromide of potassium, in doses of rather more than a drachm every half hour, is a most effectual remedy in cases of poisoning by strychnia. The physiological effects of strychnia and bromides upon the anterior columns of the spinal cord are directly antagonistic. (Dr. C. B. Gillespie, p. 365.)

SUSPENDED ANIMATION.—*Bain's and Pacini's Methods of Restoring Suspended Animation.*—In Bain's method the patient is laid upon his back on a table, and the operator standing at the head pulls the shoulders horizontally towards him with a certain degree of power, placing for this purpose the fingers of each hand in the axilla, in their front aspect with the thumbs on the clavicles. In Pacini's method the patient and operator are in the same relative position, but the operator takes hold of the arms of the patient behind, and close to the armpit, while the thumb is in front of the head of the humerus. He then pulls both shoulders towards him, and lifts them in a perpendicular direction, by which means the sternum is first raised by means of the clavicle, and, in consequence, the ribs, which, diminishing their obliquity to the spine, enlarge the thoracic cavity both in its transverse and antero-posterior diameters. A Committee of the Medico-Chirurgical Society report that by either plan, as also by Dr. Silvester's, of which they are merely modifications, a sufficiently large quantity of air is without difficulty introduced into the chest. (p. 352.)

WATER.—*To Clear Muddy Water.*—Seventy grains of chloride of calcium added to each gallon of muddy water will effect clarification in a moment. Other salts of lime, such as the nitrate and bicarbonate, and caustic lime, effect the same object. (Dr. C. Schlœsing, p. 375.)

# PRACTICAL MEDICINE.

## DISEASES AFFECTING THE SYSTEM GENERALLY.

### 1.—THE CAUSATION OF SCARLATINA.

By Dr. ALFRED CARPENTER.

[The following facts which have been met with within the last eight years are very instructive, as bearing upon the etiology of scarlet fever. The general idea on this question is that only two factors are required to produce the disease, viz., the poison, and a certain condition of the system favouring its reception. The third factor so generally required in the production of zymotic diseases, viz., some defective sanitary condition, is not thought to have much effect in the production of cases of scarlatina, as it is found to occur in gentlemen's houses perfect in every detail of sanitary arrangement, as much as in houses of a poorer class. The writer's idea is, that blood decomposing under certain states is a factor in the causation of the disease.]

Observation 1.—The children of B. B., living in a well-ventilated house on a hill side, were all seized with scarlatina in the month of September, 1864. For three or four days previously the house had been pervaded with a most nauseous smell, which proceeded from some market gardens to windward of the house. The wind had blown very quietly from the same quarter all that time. The weather had been very hot, and was moderately dry. The miasms from the gardens below seemed to hang upon the crest of the hill. There was no scarlatina prevalent in the place, and there was no known communication with any other house in which scarlatina had occurred. The servants denied that they had visited any infected houses, and the children had not been away from the hill for some time previously. Six cases occurred in that house, all commencing within forty-eight hours of each other. They were all mild in character, they ran the usual course, and each recovered in the ordinary time. The infection did not spread to others, the ordinary measures of isolation and disinfection being at once employed.

At the same time, and upon the same hill, and within the same forty-eight hours, two other families had children affected with the disease, of the like mild character, and with a similar result. The inmates of these houses were not known to each other, the children had not played together, the servants were not acquainted, they did not employ the same laundresses or the same milkmen, and scarcely the same tradesmen. Everything seemed arranged so as to make it conclusive that the miasms from the market gardens were the cause of the disease. These miasms were produced by slaughter-house refuse, with which the market gardens had been manured. The manure had been spread upon the ground as a top dressing, in dry weather, and had not been ploughed in, and at a time when there was a marked absence of ozone. Complaint was made to the local authority upon the subject. The epidemic ceased immediately the nuisance was removed by the manure being ploughed in; and no other cases, as far as could be ascertained, occurred at that place at that time.

Observation 2.—In the autumn of 1865 a nauseous sickening odour pervaded the neighbourhood of Park-hill and Croham, near to Croydon. This odour was traced to the application of slaughter-house manure to a field at Croham. This manure had been brought by railway from London, and spread upon the soil for some time before it was ploughed into it. Four or five days afterwards cases of scarlatina occurred simultaneously in three large schools in that neighbourhood, several persons being attacked in each establishment. The first cases commenced in each school within three days of each other, whilst no known intercommunication—whether by milkman, laundress, or butcher—existed between them. One school consisted of about 120 children, belonging to the Society of Friends; the second was a girls' school, about two hundred yards from the first; and the third a boys' school, about a hundred yards further on. They were all situated on different lines of sewers; and there did not appear to be anything in common to account for the attacks of coincident illness. It could not be proved that scarlatina did not exist in the neighbourhood before the manure complained of was spread abroad. A few cases had been reported in the town before the outbreak; a good many also occurred afterwards. They were not fatal, and the epidemic did not extend to any poor neighbourhood.

Observation 3.—The end of 1862 and the beginning of 1863 were noted for the number of fatal cases of scarlatina which occurred mainly in a district north and west of the East Croydon Railway station-yard. Into that yard at that time much offensive manure used to be brought from the London markets, and from thence it was carted into the country, but the trucks

sometimes used to remain some days before being removed. Public complaints were made to the local authority by the neighbours, and, after a time, the practice was discontinued. The death-rate from scarlatina has never been so high in Croydon district as on that occasion, equalling 1·64 per 1000 of population living, and 9·69 per cent. of gross mortality. This coincidence may not be due to cause and effect, but it is suggestive when it is seen that the table land around the station-yard, especially that most open to its influence on the north-west, was the situation in which most of the fatal cases occurred, though this may also to some extent have been modified by the fact that at that time very little had been done for the perfect ventilation of the sewers; and it is possible, therefore, that the prime cause might have been more rapidly spread by them.

Observation 4.—The children of G. W., inhabiting a clean, well-ventilated house at Addiscombe, occasionally became subject one by one to scarlatina. The first origin of the disease was most clearly traced to a certain room occasionally used as a sleeping room. This appartment was placed over a fowl-house, which, on examination, was found to contain much bird excrement. It was also the place in which the fowls were killed, and therefore contained blood. It was noted that the cases occurred during or after very hot nights, and markedly when ozone was absent, as on Sept. 15th, 1865, when the last case arose. The sleeping-room smelt rather stuffy, and, although the ceiling between the fowl-house and the room above was a closed one, it was evident from the odour that miasm of some kind or other did arise, and find its way into the room. After each case the room had always been disinfected, and every care taken to remove contagion; but until the fowl-house was altered, and the guano removed, the occasional occurrence of cases took place. This alteration was made in 1865. Since that time, though the children in the family have increased in number by the addition of one each year, and the room almost continually slept in, no other case of scarlatina has occurred there.

Observation 5.—A large school existed in Croydon, which on several occasions had been broken up by reason of outbreaks of scarlatina. Eventually the master gave up the house, and it became disused for a time. It was then discovered that a very large cesspool under the children's playground had not been filled up when the privy was changed into a watercloset some years before. This cesspool seemed at times to receive the washings from a neighbouring slaughter-house, and doubtless some blood by the same means. This cesspool was filled in, and the slaughter-house had its connexion made more

correctly with the sewer. The house has been inhabited again for several years by a large family—the gentleman, head of the establishment, taking pupils; but scarlatina has not appeared there since.

Observation 6.—Scarlatina became suddenly very prevalent in the village of W—— W——, four miles from Croydon, in the autumn of 1865. The disease was not known to be prevalent in the neighbourhood. It began close to the only slaughter-house in the village, and from thence spread to nearly all the children in the place. It made its appearance soon after some cleansing operations had been performed, and a great stench had been set up in the neighbourhood. The origin of this outbreak has been inferred only, as there may have been previous infection, which had been spread by the children at the village school. It is believed, however, that the slaughter-house refuse had most to do with it.

Observation 7.—The village of S—— C——, near Sevenoaks, was in a similar manner severely visited by scarlatina in the autumn of last year. The writer has no personal knowledge of this case, but he is informed, on reliable authority, that the public well is in close proximity to the slaughter-house of the district; and it was proved by analysis that the water of that well was contaminated with blood products.

Observation 8.—The writer mentioned his suspicions regarding blood, decomposing under certain states, as a factor in the production of scarlatina, to a gentleman residing in the neighbourhood. Soon afterwards, as that person rode by a neighbour's house on the Surrey hills, he observed a quantity of blood and animal offal lying on a manure heap, near to his neighbour's dwelling. He spoke to the owner, and told him that his children would have scarlatina if that stuff was left there while they played about the yard. The man laughed at him, and said it was the place at which they had always deposited the offal from the animals killed there, and that no harm had ever arisen. This was during some hot days in June, 1869. Early in July one of his children died of scarlatina. There was none in the neighbourhood. The house was at some distance from any other, and it is not known that the children who were affected had caught it from any particular source.

Observation 9.—Seven boys occupying seven different chambers in a large public school were all affected with scarlatina within a short time of one another. It was concluded that a very large number in the school would necessarily be affected, as so many centres of infection seemed at once to exist. Most admirable measures were, however, taken by the school authorities to prevent the spread of contagion, and the anticipations were not realised; for no other cases occurred in that portion of the

school. These boys were college boys, none of whom were allowed to go out of bounds, except on particular days, and then only when leave had been obtained to visit friends, The seven had, with many others, had such leave on one particular day. Scarlatina at that time prevailed in the town, and the child of a neighbouring butcher died that day. The butchers' shop had been most offensive during some preceding hot weather, and his slaughter-house was close to the confectioner's shop, around which the boys were accustomed to loiter. The authorities of the school did not make out any clear cause for the production of the disease from any other source. It appears to the writer that the sprinkling of cases thus appearing showed a source outside the school. The immediate removal of the affected to wards altogether away from the college prevented its spread among the healthy boys; and as no "leave-out" days occurred for some time, the immediate cause of the disease was removed before it could produce further mischief, and severe weather setting in stayed its further progress there.

I believe that scarlatina would be shorn of its fatal tendency if blood was not allowed to be shed in our towns and villages, except with certain precautions, so that none should enter our sewers; if our houses were built on proper foundations and well ventilated, and if pure water were always provided for the people; if sewers were so constructed as to be self-ventilating, and that continuously, and so arranged that the products of excrement putrefaction could not possibly pollute the air of dwelling-houses.—*Lancet, Jan.* 28 *and Feb.* 4, 1871, *pp.* 111, 149.

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## 2.—ON CONTAGIOUS DISEASES AND THE MEDIUM THROUGH WHICH THEY ARE PROPAGATED.

By Dr. WILLIAM F. WEST, Medical Officer to Rathmullan Dispensary, Ireland.

We may strongly presume that all contagious and infectious diseases are so through the medium of certain germs peculiar to each disease, for it is one of the proofs that a disease is contagious because others exposed to the infection are affected in exactly the same way as the patient they were supposed to take it from, modified of course to some extent by individual temperament or constitution, which goes to prove both the doctrine of Biogenesis, and also that it is through poisonous germs floating in the atmosphere that the disease is contagious. We mean by the doctrine of Biogenesis, that life is only produced from life, that is, from pre-existing living germs or organisms, as distinguished from the old idea that life might be produced from dead matter, as maggots were thought to be produced

from the tissue of decomposing meat, whereas it is now known that the maggots are produced from eggs deposited in the meat by flies. The germs of disease floating in the atmosphere and entering the system of a healthy person, produce like phenomena to those seen in the patient from whom the disease was taken; and it is easy to conceive how a disease is infectious: thus, the poison germs of the disease are exhaled from the patient's lungs and skin, and impregnate the atmosphere around with them, so that anyone in that atmosphere must necessarily breathe those germs, which enter through the thin wall of the capillary tubes of the lungs with the oxygen of the air, and through the delicate wall of the capillary blood-vessels ramifying in the air-vesicles into the circulation. The same doctrine of Biogenesis and atmospheric poison germs may also account for the various epidemics which often sweep over a line of country. Thus, certain poison germs at particular seasons of the year, generated in some way, perhaps from the eggs of insects deposited on the leaves of trees, and becoming detached as the leaves wither and dry up in Autumn, or from the decaying of vegetable matter, (which latter is likely the cause of dysentery, the fevers, typhoid, typhus, intermittent, and very likely cholera and some other diseases, and thus accounts for Autumn being the season of the year at which these diseases are most prevalent, as at that time leaves of trees and many plants wither and decay), are resolved in fact into their component elements, and in the process disseminate poisonous germs which give rise to disease; these germs being taken into the atmosphere are carried by the wind over particular districts as is seen in some epidemics, which might be accounted for by the wind blowing in one given direction, or in other cases scattered irregularly over a large extent of country, which also might be accounted for by the wind shifting from one point to another, and thus scattering the poison germs over a wider range. Medical men have argued whether epidemical diseases are also contagious and infectious, but I think in the present advanced state of medical science, and considering the foregoing arguments, it is plain these epidemics are infectious and contagious, "if, indeed, we admit the latter word as being different from infectious, for although, of course, taking the literal meaning of the word from the Latin *con*, together, and *tango*, to touch, it is certainly different, signifying, in a medical point of view, that a disease is liable to be taken by another person from the act of touching a patient labouring under such disease; yet I think there is no disease liable to be propagated in this manner from one person to another, with the single exception of parasitical skin diseases, which must of course necessarily be so, as the spore or germ of the parasite is transplanted direct

from the skin of a patient suffering from one of these loathsome complaints to that of a healthy person, wherein it finds a genial soil natural to its habits, to grow and foster itself at the expense of the surrounding tissue which it modifies or destroys. I should also say, that the diseases of insects and perhaps some reptiles are very many of them contagious, owing to the fact that, from the peculiar constitution of their skins and bodies, when certain poison germs come in contact with them they penetrate through the external tissue, and passing into the viscera of the creature, grow at the expense of its blood and so destroy it." I say, there is good reason to believe that epidemics are infectious, because the poison germs in the atmosphere that first produced the disease are not lessened or destroyed by the fact that they are taken into the system of the persons attacked, no more than wheat or any other grain is lessened or destroyed by being cast into the ground in Spring, though it may disappear from the sack or bin, or whatever it may have been contained in, and be seen no more for a time, yet it is sure to spring up again to life, bearing an innumerable offspring to propagate the species still further; so the system of living animals receiving these poison germs acts as fit soil wherever they are nourished, grow, and generate others like themselves innumerable, and are thrown off and scattered through the surrounding atmosphere, (as the grain is thrown off and scattered on the earth, or as the seed from the lofty tree or the down from the thistle is carried and scattered by the wind hither and thither and deposited in a soil wherever it grows and bears fruit), and the atmosphere near a patient thus becomes highly infectious. But the objection is brought forward that a cholera epidemic is certainly not infectious. To that I would say the infection or non-infection of cholera is still an open question, as there can be arguments brought forward on both sides: for instance, there are plenty of examples of persons obliged to be in close proximity to patients suffering from cholera, who have been stricken down, while others farther off escaped; and I heard my father tell of a house in Jamaica, right opposite to which he lived in the same street during a cholera epidemic, in which house three whole families died one after another, so at last the house was thrown down to prevent any others from going into it, which shows that the cholera poison emanating from the persons of the first family suffering from the disease had infected the house to such a degree, as to render it a habitation of death to all who entered it. On the other hand, we have examples of people nursing and coming in contact with cholera patients, and also after they died, and yet these people never took the disease; I have heard of a Dr. Geohegan, who, in the last epidemic of cholera in Ireland, used to strip himself

naked, and get into the bed of a cholera patient immediately after he died, and this he did five or six times to prove that cholera was not infectious; but I think this only proved that it was not contagious, and that there was no more danger from his doing that, than from walking in the room where the patients were; nor do I think such examples proof against the idea of its being infectious, for similar examples can be brought forward in the case of other diseases acknowledged on all hands to be most infectious, as typhus fever, scarlatina, small-pox, &c. For a disease to be infectious, it is not necessary that every one exposed to the danger should take it; if so it would be a pity for the poor doctors.

Some men, through a particularly strong and healthy constitution, have the power, to a great extent, of resisting the poison of the most contagious diseases, while others on the contrary, through a delicacy of constitution, are very liable to become victims to contagious poison. Again, through a peculiarity of constitution not depending simply on health or strength, some individuals have the power of resisting the poison of certain diseases, while they very easily succumb to that of others, just as some seeds will not grow well in certain soils, requiring a peculiar soil for themselves. In both the cases that I have mentioned, when such people are exposed to the infection of disease and yet escape any harm from it, I think even they may be infected by the poison, but through the healthy or peculiar condition of their blood, it has the power to resist those morbid changes which poison germs floating in the vital fluid always have a tendency to produce; and such persons not struck down by the disease are said to have escaped the infection, when they may be really infected, and I would say they have resisted the infection. Again, people often think they have been exposed to infection when they really have not. Thus, they go into the room of a patient lying ill with fever, measles, or small-pox or other contagious disease just at the commencement of the malady, and it is well-known that at the beginning of an infectious disease there is no poison given off from the patient, nor has it been ascertained at what particular time the poison commences first to be exhaled. It seems, "taking up my former simile," that some time must elapse for the poisonous seed to grow, generate, and ripen in the soil of the constitution before the pestilential harvest is reaped, and the grain is fully matured and exposed in the prostrate sheaves, the bodies of the victims ready to be taken to the charnel-house, the barn where human harvests are stored till the day when the Archangel's blast shall winnow like a fan the chaff and grain. A great many scientific men, however, consider that some of these contagious diseases are caused by poisonous gases emanating from the

ground in wet marshy districts, or the effluvia from sewage matter, but there is good reason to believe that it is not merely a gas that is given off in these cases, like hydrogen, oxygen, nitrogen, sulphuretted hydrogen, or any simple gas, because it is easy to understand and it has been shown that living germs are mixed, or rather are floating through those gases given off from sewage matter. And in the same way with marshy grounds, there are vegetable germs given off with the gases, perniciously modified and changed by the process of decay which the vegetable matter has undergone or is undergoing, for the humidity of the soil itself will not produce any bad consequences, a good illustration of which we have in our Emerald isle in the large tracts of bog with which Ireland abounds, and which are always swampy, yet the moisture of these bogs never produces any disease. Why is this? The Irish peasant would answer because the isle has been blessed by St. Patrick, but a more scientific reasoner than poor simple Pat would say the cause is to be found in the fact that peat has preservative properties, and arrests the decay of any animal or vegetable matter which may be in it, and likely it may have decomposing properties also. But in ordinary soil which has not been blessed by the Saint, or has not these wholesome peculiarities, the water causes or at least increases the decay of vegetable matter, and there is one very strong proof that it is from the poison thus arising that cholera is generated, which is, that in cases where cholera has broken out among our soldiers and others in India and elsewhere, those sleeping on the ground or nearest to the ground in marshy districts, are most frequently attacked, and in houses those on the ground floor suffered most, and indeed, in some cases those on the upper stories escaped altogether, while those below were dying fast—as Watson quaintly expresses it, "the cholera poison loves the ground." From this idea, that the poisons which generate disease are produced from the dying of vegetable and animal matter, is suggested another idea which I think is true, that death generates or produces death, the death of the vegetable producing the death of the animal and that of the animal producing the death of others; a fearful example of which we now have before us on the continent of Europe, where disease and death are entering rapidly the ranks of the hostile forces, from the decomposition of the bodies of their slaughtered foes and comrades. The seeds of death were first sown in our globe, when our erring mother Eve plucked the forbidden fruit, and they have been growing since, and increasing every year, till now in our time the death harvest reaped by the sickles of the various diseases is something enormous. Of course we may say this is owing to the increased population of our earth, but certainly, if the harvest field has been enlarged, death has been equal to the increased

labour, by employing an extra set of hands in the shape of new, and till lately unknown, maladies, and also seems to have enlisted the passions of men, and made their minds and intellectual powers subservient to their passions, for the purpose of inventing new machines to more speedily destroy human life, just as mowing and reaping machines were invented more speedily to cut down the corn. The march of intellect, as it is called, has certainly made rapid strides in the art of destroying life. Has it advanced as rapidly in the art of preserving it? I think this rather a doubtful question.

All that has been said with regard to contagious diseases, also goes to prove the doctrine of Homogenesis, by which is meant that like is produced from like, as contradistinguished from the doctrine of Xenogenesis, which is, that parent germs may produce an offspring different from themselves; for which latter theory was brought forward the argument that worms of various kinds in the human body, and in cattle, were produced from the tissue of the body, and were yet dissimilar from it; but this argument was founded on a mistake, for the worms in the body of a man or beast are generated from eggs deposited in it, either by the drinking of bad water, or eating badly cooked or unwholesome food containing these eggs, so this argument only proves the doctrine of Homogenesis still further, the eggs of parents producing a similar offspring. Yet the doctrine of Xenogenesis is, I think, true to some extent, as we have numerous instances of heterologous growths on and in the body, which growths must be said to be an offspring from the normal tissue of which the body is composed, or as in the case of nut-galls, an offspring from the tissue of the tree, yet differing from the normal structure of both.

These growths must be the result of mechanical or vital conditions modifying the normal structure of the body, mechanical, as in the case of corns, bunions, &c., vital, as in the case of cancerous, fibrous, cystic, &c. There are also changes found in the blood itself, the result of some morbid condition acting on the vital fluid, as is seen in phthisis, where the quantity of fat that should be in the blood is deficient, and the white and red cells are imperfectly developed, a new product is then found in the blood called tubercle, the result of this morbid condition, and which seems to take the place of the fat cells, and so the blood is greatly changed, and also in some other diseases the blood is modified pathologically, as in rheumatism, gout, leucocythemia, &c. It is plain these changes are owing to internal conditions, and so we have Xenogenesis or a parent producing offspring different from itself, and it seems that all these diseases following this law are non-contagious, which is quite reasonable, and according to our former arguments, because *that*

depending on an internal condition of the constitution cannot be transferred to another person where these conditions do not exist. So, I think none of these growths are contagious, not even cancerous tumours, which some medical men think are liable to be taken off another person, but if the pathology of cancer be true: that it depends on an internal condition of the blood, how can another take it whose blood is not in that state? and the fact that no matter how often you remove a cancerous tumour, it is liable to return in the same or some other place, goes far to prove that the disease depends upon some morbid condition of the blood itself.

Dr. Richardson objects to the germ theory on the grounds that the presence of the assumed germs is not proved, but I think he must acknowledge there are very great arguments in proof of their presence, and they have been actually demonstrated in the effluvia of sewage matter. 2. He objects that the specific character of each germ is not traceable in such malady. To this, the doctrine of Homogenesis affords an answer. 3. He objects that if the germs are reproductive and indestructible, the universe would become filled with them, which, as a media of disease would destroy the people. To this I would reply that the advocates of the germ theory do not assert that the germs are indestructible, but quite the contrary, on the idea of their destructibility found hopes of eradicating these diseases, or at least of preventing their further course when they break out, and also they are not reproductive, except when sown in the constitution of men, no more than wheat or any other grain is, except it be sown in the earth.—*Medical Press and Circular*, *Jan.* 18, 1871, *p.* 45.

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## 3.—ON THE SULPHO-CARBOLATES AND THE ANTISEPTIC METHOD IN MEDICINE.

By Dr. A. Ernest Sansom, Physician to the Royal Hospital for Diseases of the Chest, City Road; and to the North Eastern Hospital for Children.

[Perhaps no subject possesses more interest at present than the "Germ Theory of Disease." Some consider that certain living organisms or germs are the actual causes of transmissible disease, and others consider that there is not a shadow of evidence to warrant this assumption.]

We know, however, of no organic substance which can induce a train of symptoms resembling in kind and in mode of sequence the phenomena of any infectious disease; still less any poison the fatal dose of which is so minute as to be imponderable and invisible. Furthermore, we are acquainted with no simply

organic material which can be as we know the *materies morbi* to be, self-multiplying. If, then, the prime causes of disease be material organic particles, they must possess powers far transcending those with which we are ordinarily acquainted.

Some researches on forms of transmissible disease have contributed towards the elucidation of the question, particularly those of Beale and of Chauveau on vaccinia, variola, cattle-plague, &c. The researches of M. Chauveau have proved that the activity of the poisons of vaccinia, of glanders, and of sheep-pox, resides not in the fluids of the inoculated material, but in excessively minute elementary granules suspended in the fluid. He has shown, furthermore, by actual experiment, that this inoculable material may be communicated to a living body as well by the lungs and by the stomach as by insinuation beneath the skin. Dr. Beale has shown that the elementary granules of vaccine are minute masses of living matter (bioplasm) manifesting very active movements. In cattle-plague he has observed similar rapidly multiplying material in the blood of the affected animal.

In the cases, therefore, of such transmissible diseases as I have mentioned, the term "germ" has different significations.

In diseases produced by entozoa or epizoa, it denotes the transmitted ova of the animalcula specially concerned.

In diseases due to vegetable parasites, it indicates the sporules or minute granules whence the parasitic vegetables develope.

In zymotic diseases, it indicates those minute morsels of germinal matter which possess a vitality separate and distinct from the organism into which they may become absorbed, and, once gaining entrance, develope and circulate within the fluids of the containing organism. The word "germ," then, denotes nothing more than the minutest portion of living material which the mind can conceive, such material being ultimately derived in various ways from the animal or from the vegetable kingdom.

From remote times measures have been adopted having for their object the prevention or the arrest of transmissible disease. These measures have been adopted with two objects: to prevent the communication of disease from person to person, and to prevent the evolution of noxious material from fermenting and putrefying matters which have been recognised as sources of contamination. The object of disinfecting measures has been the destruction of the virus in the one case and the prevention of its evolution in the other. Materials capable of fulfilling the desired ends are very various. Inorganic as well as organic substances of endless variety of chemical constitution are capable of being good disinfectants. On a review of a large

number of these substances, and an examination of their relative power in arresting fermentation, putrefaction, and the proliferation of low organisms, one cannot help concluding that the chemical constitution or the chemical properties of a body can be no index of its position as a disinfectant or an antiseptic. They operate in no traceable chemical way; but, when they kill the organisms which accompany the fermentative or putrefactive process, they arrest all the phenomena of the process.

The question occurs, Is it possible to arrest the vitality of the germs of disease within the living body, as we have reason to believe we can destroy them when external to the body? Can there be an internal as well as an external antiseptic method? In the case of the higher forms of parasites which induce disease, this can be at once answered in the affirmative; and it affords a *primâ facie* case for the elucidation of the question in reference to the more subtle zymotic diseases. It is certain that an agent which can be taken with impunity by the higher animals can be a powerful poison to the lowest forms of life. Many of the medicines ordinarily administered, as bichloride of mercury, arsenic, the salts of iron, &c., even in very high dilution, at once kill lowly endowed organisms.

In 1857, Professor Polli of Milan, having observed the power of sulphurous acid and the sulphites in arresting fermentative processes, initiated a plan of administering the latter in cases of zymotic disease. An able paper by Dr. Polli was read at the meeting of the British Medical Association in 1867 (see British Medical Journal, November 16th, 1867). The author did not consider that the sulphites, when internally administered, acted as poisons to morbific ferments, but that they rendered the components of the recipient organism incapable of being acted on by the catalytic germs. It is difficult to understand this hypothesis; for, whether the sulphites attack the germs directly, or whether they render the soil unsuitable for them, the ultimate result is their death. It is possible that Polli agreed with Bernard, who said, "We cannot neutralise these ferments in the living organism; it is impossible, because to effect such a purpose it would be necessary to interfere with the functions of the blood to such an extent that it would be incapable of maintaining life." This would be the case if the agent acted as a poison in virtue of its chemical properties; to oxidise or deoxidise the ferment would be to gravely interfere with the functions of the blood. But we find that the most feeble oxidisers or deodorisers are the most potent poisons to low organisms; the sulphite, which can be shown at once to kill myriads of animalculæ or fungi, can be shown to exercise scarcely any deoxidising power, for it is recoverable from the body not as

sulphate, but as sulphite undecomposed. In my opinion, therefore, we should conclude that the action of the sulphite or any absorbed antiseptic would be the action of a poison upon germinal matter.

As to the success of Polli's treatment, there is discrepancy in the evidence. On the one side, many observers attested to much success. From 1861 to 1867, Polli received one hundred and forty-eight papers, which, with the exception of five or six, confirmed "in the strongest terms, by many hundreds of detailed observations, the value of the remedies." On the other hand, many who have employed them in this country have expressed themselves as disappointed with the results. From a review of the conditions, it can be at once seen that we have no right to expect from any treatment pursued with the object of arresting the course of septic poisoning, an uniformity of success. The opposing conditions are many and powerful. Common experience tells us that there may be the absorption of a morbid poison long before the manifestation of any subjective or objective symptoms, or these may be so slight as to pass unnoticed. During this incubation, there can be little doubt that collateral physical degenerations are occurring in the tissues of the body. And when the symptoms of disease have set in, a variety of induced perturbations contribute to form their complex cause; so the induced condition may be of far greater gravity than the original. Thus the ova of certain tæniæ may exist in the human body unnoticed, but their development as echinococci induces the gravest lesion. In cholera, the exfoliation of epithelium from the gastro-intestinal tract is another example of secondary disintegration. There is, therefore, abundant reason for the discrepancy between different observers. Many have probably been discouraged by a want of success at the outset. The logical conclusions are, that the value of the plan can only be determined by numerical data, and that, if it be adopted, collateral measures, suited to the various complications, should be put in force as well.

The fulfilment of the plan of treatment by the sulphites does not involve the abandonment of a plan which has been proved to have a probability of success. Our present treatment of zymotic disease is manifestly expectant. The complications which arise can be treated in the one case as in the other; and by the sulphite the functions of a saline causing the reduction of a heightened temperature are fulfilled, and something more.

Since Professor Polli's first advocacy of the sulphites, I used them in the treatment of zymotic disease, and I considered that my results confirmed his own. I felt that in the path which he had indicated there was work to be done, and that an agent might be discovered having therapeutic powers perhaps exceed-

ing those of the sulphites. Carbolic acid then came prominently before the medical world, and its powers as an antiseptic became daily more apparent. Certain difficulties attended the internal administration of itself and its simple salts; but in 1867, my attention was called by Mr. Crookes, F.R.S., to a salt which he had procured of a compound acid, sulphocarbolic, with potassium. I pushed the inquiry further, and procured the sulphocarbolates of most of the alkalis, alkaline earths, and metals, and endeavoured to study chemically and therapeutically their various characters. (See Obstetrical Transactions, 1868, p. 291; Medico-Chirurgical Transactions, 1869, p. 139; Practitioner, July, 1869, p. 5.)

The general mode which I have recommended for the preparation of salts is this: a definite sulphocarbolic acid is first formed by mixing 97 parts by weight of pure carbolic acid, liquefied by the aid of heat with 101 parts of strong sulphuric acid. Much heat is evolved, and a syrupy liquid results, which after a time becomes crystalline. Sulphocarbolic acid may, by slow crystallisation, be obtained in long colourless deliquescent needles. On neutralising the liquid sulphocarbolic acid with equivalent weight of the various bases, very beautiful regularly crystalline and stable salts are formed.

I.—Sulphocarbolates of Alkaline Bases.—Of these the sodium, potassium, and ammonium salts have been obtained. In testing their therapeutical characters, I have confined myself, for the sake of uniformity, to the sodium salt. The dose has been usually twenty to thirty grains three times a day to adults, though I have given as much as a drachm every four hours. At the North-Eastern Hospital for children, I have had abundant opportunities of testing the action of this salt in diseases of children. The patients coming from the most impoverished quarters of London, there are numerous examples of the various conditions of wasting disease. In cases of *thrush*, I have found the oidium-growth to pass away much more quickly during the administration of sulphocarbolate of sodium than under any other plan of treatment, the sulphites not excepted. There has been during the present year a great prevalence of *stomatitis*—the occurrence of small circular ulcerations upon the tongue, gums, and internal surface of the cheeks. I have noticed a very strong relation subsisting between this condition and diarrhœa. In the diarrhœa which prevails at the present time, I have, on close inspection of the mouth, been able to detect in numerous instances cicatrices indicating that a condition of stomatitis has existed unnoticed, This fact may, I think, be of subsequent importance with reference to the consideration of the causation of diarrhœa. This condition of stomatitis has invariably, under the sulphocarbolate, disappeared from the third to the seventh day of treatment.

*Follicular Inflammation and Ulceration of Tonsils.*—I have administered the sulphocarbolate in eleven such cases, with the result of complete recovery in one case on the third day; in eight cases on the fourth day; two were not observed till the seventh day—they were then quite well. Several of these cases had previously had the same affection going its course to suppurative tonsillitis—in none of them was there a trace of suppuration. I should add that they were all cases accompanied by severe pyrexia.

*Sloughing Ulceration.*—In three cases of great severity, accompanied by diphtheritic signs, there was complete recovery without complications. Convalescence took place in one case on the seventh day; in another, wherein the thermometer had registered 106 degrees Fahr., the child was able to walk on the fourteenth day; in the third, with severe pyrexia (temperature 105 degrees Fahr.), delirium, and large ashy slough over each tonsil, perfect power of swallowing returned on the sixth day, and progress was uninterrupted.

*Scarlatina.*—In eighteen cases in which the sulphocarbolate was administered, recovery was very rapid. The throat-signs became rapidly alleviated; it was an almost invariable rule that, at the end of the fourth day of treatment, all throat-distress had passed away. It should be added, that in all these cases not any local treatment to the throat whatever was practised, nor any other remedy save the sulphocarbolate. The pyrexia rapidly diminished. Complete convalescence took place in six cases in seven days; in one, in eleven; in one, in fourteen; in one, in fifteen days. The sequelæ were, in one case general anasarca resulting in recovery; in one, albuminuria for a single day; in one, abscess of a superficial gland in the neck; and, in one, a persistent slight glandular swelling.

*Variola.*—My experience of the remedy in this affection is very limited; but I think it is sufficient to induce an extended trial. I have used it in two cases; one of these was a gentleman of middle age, who made an excellent recovery. He had, however, been vaccinated, so the result could have but little weight. I have since, however, had a severe case in a child of one year and eight months, unvaccinated. The progress was uninterrupted, the pitting very slight, and complete convalescence took place at the end of the fourth week. In none of the zymotic cases thus treated did a death occur.

*Enteric Fever.*—I have not myself had any opportunity of using the sulphocarbolates in this disease. Dr. Ligertwood, of Newbury, has, however, put the plan in practice, and has forwarded me a tabular statement of twenty-four cases so treated. Dr. Ligertwood says: "I think the treatment was very successful. There did not seem to be the same tendency to

relapse that I have found occur under other treatment. The diarrhœa, often very severe, never became so exhausting as to call for any special treatment, and the average duration of the fever was certainly not more than under the usual modes of treatment."

*Tuberculosis.*—In testing the therapeutic effects of sulphocarbolate of sodium upon phthisical patients at the Chest Hospital, where I first begun to employ it, I was so struck with the signs of general improvement that I determined to give it an extended trial in these cases. It must be remembered, however, that in most cases cod-liver oil was used in addition. In eleven cases of children showing signs of general tuberculosis, eight improved considerably; two seemed uninfluenced; and one went from bad to worse, the progress of tuberculisation being evidently unchecked. From notes of seventy-eight cases of pulmonary phthisis in which I employed it, the following are the results. *First Stage:* fifteen cases. In eleven much improvement, with marked decrease of symptoms and increase in nutrition. *Second Stage:* fifty-one cases. Thirty manifested much improvement; seven slight improvement; twelve uninfluenced; in two disease progressed rapidly. *Third Stage:* sixteen cases. Ten much improvement; twelve uninfluenced; in two disease progressed rapidly. I entirely leave the interpretation of these results.

II.—SULPHOCARBOLATES OF THE ALKALINE EARTHS.—The *magnesium* salt crystallises in large clear rhombic prisms. One of the most interesting of the whole series is the *calcium* salt. This is obtained in long, fine, delicate crystals, which, interlacing, somewhat resemble benzoic acid. It is exceedingly soluble; and this I consider to be a most valuable characteristic. The following expresses the solubility of calcium salts at 60 deg. Fahr.

| | | |
|---|---|---|
| Calcium carbonate | } | insoluble. |
| ,, phosphate | } | |
| ,, hydrate..... | | 1 part soluble in 800 of water. |
| ,, sulphate ..... | | ,, 500 ,, |
| ,, sulphocarbolate. | | ,, 1 ,, |

I thought that the calcium sulphocarbolate might prove useful in the treatment of rachitis. I accordingly employed it in twenty-six cases, with these results: great improvement in sixteen cases (of these there was, to all seeming, complete recovery in ten); moderate improvement in five; doubtful result in five.

In some of the cases cod-liver oil was administered in addition; but in many, in which there was the most marked improvement, the sulphocarbolate alone was given. The usual dose for children between the first and second year was five grains. I have also used the sulphocarbolate added to ordinary

chalk-mixture in cases of chronic diarrhœa in unhealthy children, apparently with good result. Of the efficacy of the treatment in rachitic disease I think I am entitled to speak with some confidence.

III.—SULPHOCARBOLATES OF THE METALS.—Of these I have obtained the iron, the copper, and the zinc salts, all of which present highly characteristic appearances. The zinc salt is in brilliant colourless right rhombic prisms; the copper salt possesses a blue colour, much resembling that of the sulphate. Mr. John Wood has employed these in lotions to prevent suppuration in wounds: he considers that they exercise an obvious control over the suppurative process. The only salt of the internal administration of which I have had any experience is the iron sulphocarbolate. This is a colourless very soluble salt, crystallising in rectangular plates. It assumes, in the presence of peroxide of iron, a deep mauve or purple tint. I employed it as a general tonic in diseases of debility in children; some cases of eczema and of impetigo became rapidly better under its influence, but I did not conclude that it was superior to the other salts of iron. In a case of enlarged and strumous glands, there was much amendment, and the swellings almost entirely disappeared. In ten cases of threatened tubercle there was considerable improvement in four; in the remainder it was doubtful. In cases of pulmonary tuberculosis there was much improvement in six cases; slight improvement in seven cases; no improvement in eight cases. I could state confidently that the results were not nearly so satisfactory as in the cases treated by the sodium salt.

In conclusion, I trust that these few imperfect observations will induce many of my professional brethren to test in actual practice the value of these interesting salts. Their place in therapeutics can in no sense be fixed by the experience of one individual. As to their mode of action, there may be very many who widely differ from me in the opinions which I have enunciated; but their physical characters alone will, I think, enable them to hold a useful place in our materia medica.—*British Medical Journal, Dec.* 24, 1870, *p.* 677.

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## 4.—ON THE FEVERISH STATE.

By Dr. LIONEL BEALE, F.R.S., Physician to King's College Hospital.

Let us inquire into the nature of the changes which mark the feverish state, and endeavour to decide upon what principles our treatment of this common morbid condition should be based. Feverishness may be induced by many very different circum-

stances, and varies in intensity from a very slight derangement of the normal physiological changes, of little consequence, to a condition that must end fatally in the course of a few hours.

The invasion of almost every form of acute disease is marked by feverish symptoms; and there are not a few chronic diseases which are accompanied throughout their whole course by fever. Feverishness may be studied in the Surgical as well as in the Medical wards of the Hospital; and there is certainly not an individual in any civilised community, and probably not one even among savages, who has not experienced, at least in some degree, the phenomena of feverishness in his own organism. Nor are any warm-blooded animals exempt from fever. It is probable that even cold-blooded vertebrata suffer; and I could adduce evidence to show that some of the changes characteristic of fever may be induced even in the frog. Slugs and snails and worms suffer some of the changes characteristic of the feverish state. Insects, probably owing to their organisation, cannot be feverish; and certainly the lowest animals and plants are free from any general morbid change allied to fever.

Now, the living organism which is the seat of a slight feverish attack, differs only from the perfectly healthy body in the fact of the accelerated rate at which some of the ordinary physiological actions are being carried on, and the reduced intensity of other constant processes.

The change may be very temporary, lasting, perhaps, only for a few hours, and neither accompanied nor caused by any structural change in any tissues whatever. At first the blood alone is affected, and a little increased action of the lungs, skin, bowels, or kidneys may at once cut short the slight disturbance which has taken place in the circulating fluid, and which has occasioned the fever—so far a *functional derangement only*.

In very slight fever the heart beats more frequently than in the normal state, but it by no means follows that the blood is made to circulate more quickly through the capillaries of the tissues and organs of the body. On the contrary, there is evidence in many organs of impeded circulation, of obstruction to the free passage of the blood through the capillaries, of distension of their walls, and in some of accumulation of blood. Consequently, the animal heat is not carried off as fast as usual, and the temperature of the blood rises. At the same time there is in many parts of the body, even at an early period of the disease, an increased development of heat. The exalted temperature in fever is, indeed, due partly to the disturbance of the processes concerned in the *carrying away of heat*, and partly to the *increased activity of the heat-producing process*.

Less water is given off from the blood in the feverish than in the healthy state. The blood is, therefore, of lower specific

gravity than in health. Many of the little arteries are dilated, and, the capillaries being distended, the blood flows through them more slowly, and undergoes less change as it flows than in perfect health. The pressure upon the vascular walls is increased. The feverish patient is even conscious of a *feeling* of fulness and distension in many parts of his body. The muscles seem tired; the skin is dry; the lips parched; and the mucous membranes less moist than they should be. All the excreting organs—the liver, the skin, the kidneys, and the bowels—work less actively than is their wont. Although the temperature of the body rises, there is a feeling of chilliness—perhaps actual shivering, as from cold—an instinctive desire for external warmth, yielding to which will, indeed, bring ease. A warm bed or a warm bath will relieve all the unpleasant sensations accompanying a slight febrile attack in a very short time; but they recur if the external heat is removed before recovery has taken place.

The phenomena characteristic of the feverish state we are discussing, and some others, the consideration of which I have purposely omitted, are consequent upon changes in the blood. A perfectly healthy state of the blood and the feverish condition cannot coexist, and it is impossible for fever, even of the mildest degree, to occur without accompanying changes in the composition of the blood. Indeed, the blood-change is the starting-point of every feverish attack, and, if this be due to temporary disturbance only, the balance of opposing actions is soon restored by compensation, and health returns; but if it depend upon circumstances which cannot be quickly changed, the feverish state continues, and the degree of its intensity increases.

It has long been known that when fever, inflammation, and other blood diseases have become established, the composition of the blood is altered, and even in a slight feverish attack which constitutes an ordinary cold, the chemistry of the blood is temporarily deranged. The extractive matters soluble in boiling water are present in undue proportion, and it is probable that this increase arises from insufficient oxidation. Various matters, which in perfect health are very highly oxidised, so as to be eliminated in the form of carbonic acid, urea, and other substances which are readily excreted, there is reason to think, remain in the blood imperfectly oxidised, or are very slowly and with difficulty eliminated in a suboxidized state. Thus there remains in the circulating fluid an excess of soluble material, which permeates the tissues much more readily than ordinary healthy serum would do. This soluble matter transudes through the walls of many capillaries, and is appropriated by the bioplasm of the blood, of the vessels, and of the

*tissues*, to their detriment. These very substances are susceptible of a higher degree of oxidation, and might have been oxidised and excreted as urea, carbonic acid, and other fully oxidised substances. The bioplasts or masses of germinal matter invariably increase in size in every form of fever. By this increased growth of germinal matter, which invariably takes place in all inflammations as well as fevers, the close analogy existing between these two classes of diseases is established.

One important change in the composition of the blood when fever has been induced, may be demonstrated in a very simple way. If the dried residue of the fever blood be extracted with boiling distilled water, it will be found that the proportion of matter dissolved out from the fever blood is much larger than that obtained from the healthy blood residue. Three specimens of blood taken from animals which died of the cattle-plague fever contained, respectively, 2·91, 2·22, and 1·81 parts of soluble matter dissolved out by boiling water, or twice the quantity extracted from healthy ox-blood. The exact amounts were as follows :—

| | Healthy Ox-Blood. | | Blood from Cattle Plague. | | |
|---|---|---|---|---|---|
| | 1. | 2. | 1. | 2. | 3. |
| Solid matter obtained by evaporating 100 parts of blood ... | 19·87 | 20·63 | 23·1 | 22·78 | 24.88 |
| Substances soluble in boiling water ... | 1·33 | 1·11 | 2·91 | 2·22 | 1·81 |

The solid matter of the two healthy specimens contained, respectively, 6·69 and 5·38 per cent. of matters soluble in boiling water, while the diseased specimens contained, respectively, 12·62, 9·72, and 7·22 per cent. So that not only is the percentage of the *solid matters* generally greatly increased in this form of fever, but the extractives and other substances soluble in boiling water are present in increased proportion. These substances probably constitute a pabulum, which is very readily appropriated by degraded forms of bioplasm.

On the other hand, it has been insisted upon by many—but upon purely theoretical grounds—that fever results from per-oxidation. So far from this being so, it is certain that the feverish condition is from the very first associated with diminution in the activity of the oxidising processes. I even doubt if there is any evidence whatever in favour of the view that any morbid state known to us depends upon *too free oxidation*. I do not believe that hyperoxidation is possible. I doubt if we *can* oxidise ourselves too much. The greater number of our ailments—temporary and permanent, slight and severe, trivial and fatal—are unquestionably due to, and are associated with, the very opposite condition—insufficient oxidation.

Although fever and inflammation are often considered as if they were very different conditions, the essential pathological phenomena are the same in both states, and a fever may be looked upon as a general inflammation, while an inflammation may be correctly regarded as a local fever. Fever may usher in a terrible inflammation, and a local inflammation may give rise to general fever. The minute changes which may be so readily demonstrated in ordinary inflammation, have much in common with those that take place in fever, but in the former condition the phenomena may attain a stage which in fever could not possibly be reached, for the simple reason that death would occur first. A part of the body may be destroyed by the growth and multiplication of the living matter, or bioplasm, until pus results, and although universal suppuration is, of course, impossible, we do actually meet with cases in which fever runs high and lasts long, and has actually led to suppuration over a great part of the body.—*Medical Times and Gazette, Feb.* 18, 1871, *p.* 183.

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## 5.—ON A LOCALISED OUTBREAK OF TYPHOID FEVER IN ISLINGTON.

By Dr. EDWARD BALLARD, Medical Officer of Health for Islington.

[The outbreak of fever which forms the subject of this paper, occurred in July and August, 1870. Various suggestions were made as to its cause. Some thought it was owing to a new railway cutting, opening the mouths of a number of old drains and sewers, but the cutting extended in both directions far beyond the boundary of the invaded district, and yet no typhoid developed except in that district. Others thought it might be a dung-shoot in a yard, but this is overhung on three sides by dwellings occupied by labourers, yet in these not a single case of fever occurred.]

Another suggestion made to me proceeded primarily from a lady, secondarily from her Medical attendant, to whom she expressed her idea. Fever occurred in this lady's family, and in four families of her acquaintance about the same time. She then called to mind that some little time previously she had changed her milkman, and, as ladies will do, had induced those friends of hers to adopt the same tradesman. *Ergo*, it was the milk. It happened that her Medical attendant, during the months of July and August, had several families of his *clientèle* down with typhoid, and, among the rest, the family of the very milkman whose wares were suspected. He wisely made some cautious inquiries, and found that out of eleven such families

there were ten who were supplied from this source; the exceptional family resided a mile and a half off. Comparing notes with other private practitioners, the reality of a serious outbreak became obvious. Till then I had only heard of fatal cases, but now these other gentlemen began to make a similar inquiry, and there was such uniformity in the result that one after another communicated with me upon the subject. Altogether, private lists of cases were furnished to me by nine private practitioners in the neighbourhood, marking the families supplied by this milkman—some spontaneously, others immediately on my request. At first I confess I was sceptical; I thought I could satisfactorily account for the cases, without having recourse to what appeared a rather far-fetched theory; albeit, it became the favourite one with my medical friends, each, of course, guided by his individual experience. I thought, considering the limitation of the outbreak, and the probability that a milkman in the neighbourhood would supply a great part of it, that, after all, the association observed might be nothing more than a coincidence, strange and curious indeed, but of no scientific worth, and I did not hesitate to check gossip by saying so. When I add that now I not only believe that the distribution of milk from the dairy I have mentioned was the means of sowing typhoid in the district, but regard it as proved, it may be readily conceived that the evidence I have gathered is tolerably convincing.

The general result of an inquiry into the milk-supply of the families within the quarter mile radius into which I ascertained that typhoid fever had entered was this, viz.:—

That it occurred in the house of the milk-vendor, where the business was carried on; he died; and seven other persons, members of his family, or boys employed and living on the premises, had typhoid, and one of the latter died.

That it occurred in the family of a person who dwelt in a small cottage in the cowyard, distant about 100 yards from the last-mentioned house and dairy, three persons having fever here. The first case here was in a girl, who, a fortnight before she actually fell ill, had left a situation at a public-house supplied with milk from this dairy, and since then had been residing at home. This family, the mother of the girl told me, rarely took milk, except on Sundays, being poor; but when the girl was ill, milk was given to them from the dairy. The mother herself, and subsequently a little boy, had typhoid when the girl was convalescent.

That, in addition to the two boys who had lived in the house for a longer or shorter time, two men engaged in the business, who did not live in the house, had typhoid, One of these was the cowman, engaged after the master was taken ill; the other

was a young man who worked at the cowyard and carried out milk, and who took all his meals at a coffee shop supplied from the dairy, and where two families, also supplied from the dairy, had fever.

That a fifth man employed at the cowyard, and residing at home, had fever. He also occasionally had his meals at the above-mentioned coffee shop, and his tea at his mother's, who was supplied from the dairy. His family being poor, he bought no milk for them anywhere, and he was the only member of it attacked.

That another single man, residing within the radius in a wholesome house, and engaged as a coachman outside the radius, who habitually took his meals at this same coffee-house, had an attack of typhoid.

That (omitting the girl who came ill from the country), out of sixty-two other families within the district which are known to have suffered from typhoid, fifty-four, or fully 87 per cent., were constantly supplied from the dairy with the milk they required, two were occasional customers, and five only stated that they did not deal there at all. I am not quite sure that complete reliance can be placed on this last statement; certainly I doubt it in one case of the five. All five resided close to the dairy, and it is most probable obtained there, as the most convenient place, any extra milk their families required. In another instance, a girl was attacked in a family not supplied by the dairy, but it appeared that, on two consecutive days in the beginning of July, she had taken tea with a schoolfellow, who, with her mother, had typhoid a few days later, this family being supplied from the dairy. No one else in this girl's family had fever.

Confining myself now to the *fatal* cases happening in families who resided within the quarter mile radius (some of which individuals, however, were taken ill or died in the country), but excluding the girl who came ill from the country and died within the radius, I find that the total of such cases was twenty-five. Every one of the families in which these deaths occurred dealt for milk with the dairyman in question—[I include here the deaths in his own household]—only one of them was an occasional customer.

Outside the radius, in the rest of the parish, I said I had fourteen fatal cases to account for. Five of these fourteen occurred in families supplied by this dairy—that is, more than a third of them. With such facts as these before me, I had a strong *primâ facie* case against the milk; but, strong as it was, it did not absolutely exclude the idea of mere coincidence. I did not know how far the dairyman's connexion extended. I learned, however, that customers of his outside the radius, some

at the distance of a mile or more, had been attacked with typhoid, and that several of them had died. What I had now to do, therefore, was to ascertain what families he actually did supply; with this information I should be able to determine the question decisively. Accordingly, I communicated to the father of the deceased dairyman the rumour which was freely canvassed both in the Profession and in the neighbourhood, and suggested that it would assist in settling the question if he would consent to furnish me with a list of his son's customers. It redounds greatly to his honour that he at once complied with my request, and expressed his desire to do everything in his power to assist in unravelling the mystery of the outbreak. I may say here, as the proper place for saying it, that he fully carried out his promise, and in doing so has earned not only my personal respect but the sympathy and respect of every right-minded person.

The result of my inquiry in this direction is that the members of 142 families were supplied with milk from the dairy in question. The district within the quarter mile radius alone must contain over 2000 families. So, after all, no very considerable proportion got their supply from this source. Out of these 142 families (which includes the dairyman's household), I have ascertained that 70 were invaded by typhoid within the ten weeks during which the outbreak extended—that is to say, half of them were invaded. This includes all the families which had deaths from typhoid. I did not go from house to house to inquire among all the customers, so that it is quite possible that other non-fatal cases may have occurred in other families whose Medical attendants have not communicated with me. But this is enough for us. Inquiring as to the source of milk-supply in all the other instances of typhoid in the parish, during the same period, that came to my knowledge, I scarcely ever heard the name of any one milkman mentioned twice.

In these 70 families there happened altogether 175 cases, of which 30 were fatal—that is, the fatality was 17·1 per cent.

Again, taking the list of customers, I have gone through my mortuary records with it house by house, and I find that, besides the thirty deaths from typhoid which I knew of, there were altogether but three other deaths in these families—one was of an infant born prematurely (from what cause I could not ascertain), one was of a person who died suddenly with fatty degeneration of the heart, and the third was of an old lady, whose death is attributed on the certificate to "choleraic diarrhœa."

Probably, enough has been stated to satisfy you, as I am satisfied that the distribution of the typhoid was connected

with the distribution of the milk from the particular dairy referred to. But, at the risk of being charged with heaping Ossa upon Pelion, I shall mention some other observations which add confirmation to the inference. They are interesting apart from this use of them.

1st. It is remarkable how the typhoid picked out, as it were, the customers of this dairy in particular streets and rows of houses. Thus, in one long road, and a street issuing from it, at a distance of a mile or more from the dairy, it supplied three families; of these, two had typhoid. It supplied two families in a street with about thirty houses; one suffered from typhoid; in the other died the old lady already mentioned from "choleraic diarrhœa." It supplied four families in a new neighbourhood with about seventy houses; three of these families had typhoid. It supplied four families in a crescent with twenty-five houses; all four had typhoid (in one only a single mild case occurred). It supplied four families in a row of nine houses; typhoid occurred in two of them, and, in the other two, cases of a mild febrile character (not enumerated) occurred. It supplied four families in two opposite rows of houses, altogether about sixty-seven; three of them had typhoid happen in them. It supplied four families in a square with fifty-nine houses; all four had cases of typhoid happen in them, &c. And these were, so far as I can ascertain, the only cases in these several localities.

2nd. It is to be observed that there were comparatively few solitary cases in the 70 families. Solitary attacks only happened in 22 of them; and these 22 include the three individuals who alone, of all the inhabitants of the same house, had the opportunity of getting their disease from the use of this particular milk. Of the 67 invaded families in which several members were exposed to the same risk, there were, then, only 17 in which solitary cases occurred. The attacks were multiple, then, in 75 per cent. In these families, the number of cases varied from 2 to 8 in a family, the average being a little over 3 cases per family. All will agree with me that this highly multiple character of the attacks in families is a feature not commonly met with except under circumstances where all the members of a family have been exposed to a common cause.

Still, it does not follow that where multiple attacks thus occurred every case in a family was due to the use of the milk and that alone. Probably in some instances the disease, being once introduced into the house and implanting there the contagion through the discharges, spread in the family and household, as it often does under such circumstances. This was very likely the case in some of those instances where an interval of two or three weeks elapsed between the first seizure in a house

and the subsequent ones. In favour of this view I may state that in one instance in which typhoid contracted from this milk was developed in the country, it spread in the family and house to which the patient had migrated. In one instance a servant went to her home in Islington ill, and her father there was attacked. I have not been able to ascertain, although I have inquired, that a similar event happened in any other instance. But if this explanation held good to any great extent, we should have expected that the multiple character would have been as strongly marked in the domestic invasions occurring late in the outbreak as in those occurring early in it. On the contrary, however, as the invasions of houses supplied with this milk became delayed, so the multiple character of the domestic attacks became less marked. Thus—

| | | | | | Total cases at intervals. |
|---|---|---|---|---|---|
| In the | 1st | week | 9 houses | were invaded | 32 or 3·5 per house. |
| ,, | 2nd | ,, | 16 ,, | ,, | 52 or 3·2 ,, |
| ,, | 3rd | ,, | 13 ,, | ,, | 36 or 2·7 ,, |
| ,, | 4th | ,, | 6 ,, | ,, | 17 or 2·8 ,, |
| ,, | 5th | ,, | 6 ,, | ,, | 9 or 1·5 ,, |
| In subsequent weeks | | | 18 ,, | ,, | 29 or 1·5 ,, |
| | | | 68 | | 175 |

There was, moreover, no instance within the quarter mile radius in which two families were attacked in the same house, except where both families used the same milk. On the other hand, there are several instances in which, more than one family residing in a house (the different families using milk from different purveyors), that using the milk from the dairy in question alone suffered.

3rd. The attacks in families appeared to have some relation to the quantity of milk consumed. It is a matter of common remark that milk is more largely consumed by females than by males, and by children than by adults. Now, leaving out the young children attacked in the families supplied from the dairy, and taking those over 10 years of age and the adults, I find that, while 97 females had the fever, only 44 males had it—that is, females formed nearly 69 per cent. of those attacked above 10 years of age. I am not aware that, under ordinary circumstances, typhoid shows any such remarkable preference for the female sex. Again, out of 47 families in which multiple attacks were observed and the date of the first case determined, it occurred—

| | | |
|---|---|---|
| In a woman over 20 years of age . . | 20 | times. |
| ,, child under 10 years of age . . . | 12 | ,, |
| ,, girl between 10 and 20 years of age . | 6 | ,, |
| ,, man over 20 years of age . . . | 6 | ,, |
| ,, lad between 10 and 20 years of age . | 3 | ,, |

The first attack in the house only occurred in a man or youth nine times out of forty-seven. Some curious illustrations of this relation came out under the inquiries instituted at individual houses. Thus, in the family living at the cowyard, the daughter, while engaged as a nurse in a situation, alone got the dairy milk with regularity, and she was the first attacked; the second case in this house (her mother) was a month later. In a family consisting of the father and mother, who never took any milk at any time, two servants and four children all had typhoid, except the father and mother, the children commencing. In another family, consisting of mother, two servants, three girls, and a boy of 17, one of the girls and the boy took milk porridge daily at breakfast; the other girls, with the mother, took little milk comparatively. The servants, complaining that the beer was sour, asked permission to take milk instead of beer. The girl and boy who ate porridge and the two servants were alone attacked. In another family, where a daughter aged 18 and a son aged 5 years were attacked, the daughter, I was told, was a great drinker of milk, and she was attacked a fortnight before the son. In a house occupied by several families, using one privy, and where the drain-smells from an overfilled cesspool were very offensive, only one elderly man and woman were attacked. They alone drank milk from the dairy; the other families, being poor, had never any milk at all, and altogether escaped. In another family, the only person attacked was a young girl, who, being in delicate health, took more milk than all the rest of the family. Mr. Clifton also told me of a case of typhoid, which is not enumerated here, in the person of a young lady whose family was supplied by some other purveyor, but who fancied to drink daily a glass of milk from the dairy in question. No one else in the house ever took this milk, and she alone suffered.

I have very little more to add about the cause of the distribution of the disease. I have stated that miasmata of local origin played a part in the etiology of the outbreak. Such local miasmatic conditions were discovered in or about 23 out of 68 houses in which fever occurred in connexion with the use of this particular milk. This number is equivalent to 33 per cent. Now it is well worthy of remark that the houses thus conditioned were on the whole those attacked earliest in the outbreak among the customers of the dairy. Thus, of the 25 houses invaded in the first fortnight, 15 were thus circumstanced; of the 19 in the second, 7; of the 16 in the third, 1 was dirty and unwholesome; of the 8 at later periods, all were free from local miasm.

There can be little doubt that although these unwholesome

conditions proved of themselves insufficient to produce typhoid in the persons exposed to them, yet that such exposure imparted a disposition to suffer from the contagium introduced into the system by the stomach.

These 23 houses had 83 cases amongst them; in only 2 of them were there solitary cases of fever. The average in the 21 multiple invasions is, then, 3·8 cases per house instead of 3 cases, as was averaged in the multiple invasions generally. And the fatal cases were 12—that is to say, the fatality under the combined influence of the milk and miasm was at the rate of 14·4 per cent.—which is lower than the average fatality. The inference being, that although the combination forced, as it were, an attack of typhoid on some individuals who probably would not have suffered from the milk alone, the attacks such individuals underwent were comparatively mild.

*How did the typhoid contagium get into the milk?* In considering this question we shall be considering the origin of the outbreak.

1. It is clear that it must have got into it either before it was sent out from the dairy or subsequently. The only probable way in which it could have got into the milk in its passage from the dairy to the customers is by the addition of foul water by the milk-carriers, in pursuance of a fraud upon their master. But against this supposition we have the fact that the young man who managed the dairy business—the master, indeed (who was one of the first fatally attacked)—himself carried out the great part of the milk in a cart, and also the fact that the disease, even in the first week of the outbreak, was so widely sown as to preclude the idea of such a fraud having been perpetrated unless by general concert between the master and the men. Neither will it serve to explain the fact that the family of the dairyman was itself attacked very early in the outbreak. The milk that they got is not to be supposed thus fraudulently contaminated by a milk-carrier outside the premises.

2. But, besides sending out the milk furnished by his own cows, this dairyman often bought milk for sale from other sources. Sometimes, when the demand was greater than his supply, or when two or three of his cows ran dry, he bought considerably. Was this milk that he bought and served out to his customers together with his own that which contained the contagium? I inquired where the milk under such circumstances was obtained, and the answer was, from neighbouring cowkeepers, and several that I know were mentioned to me. This was quite sufficient to clear the purchased milk from suspicion; because, if that supplied from any one of these dairies had been the dangerous milk, I should have

found the principal cases of typhoid in families supplied by that dairy. Now, as a matter of fact, in my inquiries throughout the parish into cases occurring during the whole ten weeks, I scarcely heard the name of any other dairyman mentioned twice.

3. If, then, we ask *where* the milk obtained the contagium it held, as a question preliminary to *how* it obtained it, the answer must be—Upon the premises of the dairyman. My investigation was now localised, and the question came to be—What possible sources of typhoid contagion existed upon the dairyman's premises? And now I must tell you something about their geography.

The cowhouse was situated on one side of a large open yard, communicating with a narrow lane, at the entrance of which were the stables and dwelling-house of a cab proprietor, into whose family, although supplied by the dairy with milk, typhoid did not enter. With this trifling exception there were no houses very near it; its neighbourhood was mostly unoccupied land and gardens. The cowhouse formed part of a range of stable-buildings on one side of the yard. On the adjoining side was a row of buildings used as warehouses. The cowhouse was well paved and always kept clean. There was no drain but the surface one, which conducted liquid matters along a channel to the centre of the yard, where they entered a gully opening, and passed away into the drains. The stables were drained by pipes towards the same gully, the pipe being continued into a small sewer in the lane. The dung was stored against the wall of the stable. I have no record of any complaint being made at any time against these premises on the score of uncleanliness. They were always clean when I have seen them. Ten or eleven cows were kept here. At the time of the outbreak, and for a long time before, there had been no disease among them—they were a fine and valuable lot of cows. The young man who carried on the business up to the time of his illness milked them himself, and saw to their being properly kept. In the centre of the yard, the paving of which had, from the heavy traffic upon it, got a good deal out of repair, was a large heavy gully grating, which it was always believed was properly trapped, but which, on examination in the course of the inquiry, was found not to be so. Beneath it was a receiver about three feet deep, from which the drain passed. I perceived no offensive smell from it, nor can I hear of any one who ever noticed any; but on gauging it with a fork, a violent gush of offensive gas was evolved, almost explosively. On the side of the yard opposite the warehouses was a cottage of two rooms, occupied by one of the workmen, his wife, daughter, and little son. All but the man had fever. The daughter was taken ill the same day as the dairyman—namely, on July 9.

This was the girl I have mentioned who had left a situation as nurse, where the milk in question was supplied a fortnight before her illness. Above the cottage was a large open iron tank of New River water. When I saw it, it was foul. It had no waste-pipe. It was used for the cows and horses, for washing vans, and for all domestic purposes by the occupants of the cottage.

The milk business, however, was not carried on here, but at premises about a hundred yards off, to which the milk was carried in the cans prior to distribution. I am assured that none of the water from the tank over the cottage was ever added to it.

The premises where the milk business was conducted consisted of a dwelling-house, covered yard, stable, and offices. The arrangement of these is shown on plan (*see Med. T.*, p. 616). With respect to the stable, I must say that it was large, airy, admirably paved with iron bricks, and always very clean. It was provided with pipe drainage beneath, the channel entering an old brick drain (B), and with patent stable traps. Prior to the outbreak in these premises I am informed that some of these traps were broken, and that the smell was very offensive, and diffused into the yard beyond, especially when a closet, used by the workmen on the premises, which communicated with this drain at the further end of the stable was flushed. During the illness of the dairyman at this house, the new traps were put in, and I am informed there was no accumulation in the pipes. At the side of the stable was a cart-shed, and near its entrance a wooden and glass office, attached to which, on the outside, was a flap-table, above which, on the woodwork of the office, the cans used in the business were hung up. The kitchen of the dwelling-house, projecting backwards, had a window looking upon this table. The water-closet was situated in a narrow strip of yard at the rear of the kitchen. There were two receptacles for water on these premises. One was a cistern situated in the back yard. It was closely and well covered, and when examined found to be scrupulously clean. The waste-pipe did not pass into a drain, but discharged upon the surface of the paved yard. From this cistern a pipe proceeded to a tap at the side, and a little below the level of the flap-table mentioned. The other was an underground tank, situated beneath the flag-stone paving of the office against which the flap-table was erected. The water was drawn from it by means of a pump situated at the entrance of the stable from the yard. I may mention for what the fact is worth, that this pump, although in view from the office, was not in view from the window of the kitchen looking upon the flap-table. This tank, like the cistern, was supplied from the mains of the New River

Company. It was constructed about sixteen years ago, and during that time no examination had been made of it. It had, however, been opened occasionally when it became dry from fixing of the ball-cock. But the time had now arrived for a thorough examination of it to be made. This examination, however, from the absence of the occupier of the premises during the convalescence from his attack of fever, had to be delayed, and it was not thoroughly examined until the beginning of November. There was no objection to this delay, inasmuch as the death of the young man who carried on the business led to the trade being given up, the cows being sold off very shortly after his death, which took place on Aug. 12. I may mention here, again, for what it is worth, that only eight fresh houses amongst the customers of the dairy were invaded after this date, six of them in the fifteen days between the 12th and the 27th, and only two later.

We may ask, then, which of these two sets of premises—the cowhouse and yard, or the business premises where the milk was sold—furnished the contagium to the milk? To take the cowyard first, there were present as possible sources of typhoid effluvia the foul receiver in the centre of the open yard, and the foul water tank over the cottage. I think we may absolve the latter at once, inasmuch as it is improbable that water would have been added to the milk here while there was plenty at the place of business whither the milk was to be carried. With respect to the foul receiver in the yard, I should place little value on any objection to it as a probable source of contagium on the mere ground of offensive effluvia not being noticed, so long as it could be shown on independent grounds that typhoid fever had actually been contracted by any person or number of persons from exposure to its emanations, or that it was probable that the milk had been sufficiently exposed to the chance of absorbing the contagium proceeding from it. It seems to me that any emanations from the gully would, before reaching the cowhouse, have been too diluted to produce mischief in this way; while it is understood that the only other possible exposure it could have, would have been during the carriage of the milk through the open air near it. Was there, then, any independent evidence of the gully or receiver actually giving off typhoid contagium into the atmosphere of the yard? It was a yard in which a good deal of business was carried on. Horses were stabled there, vans were washed near the gully, &c. As a fact, people employed about this yard were attacked with fever. Thus, 1st, one of the earliest, if not the very earliest case, happened in the person of a boy who was a "useful" boy about both premises—in the stables and at the cowhouse and yard. He was so ill on July 3 that he was sent home to his mother, at

Finchley, where he died of typhoid on July 25. 2nd. The dairyman himself, who personally milked and managed the cows, spending much of his time in the yard, was attacked early—namely, on July 9. 3rd. The young woman at the cottage in the yard was attacked the very same day. 4th. A man who worked in the yard habitually, chiefly about the stables there and on the business premises, washing vans in the yard, &c., but who had nothing to do with the cows or dairy business, was attacked with fever early—viz., on July 7. 5th. Another young man, similarly occupied, was attacked on August 10. 6th, and lastly, a cowman, who came newly to work at the yard and to take the place of the master, who was ill, in milking and managing the cows, about the beginning of August, was taken ill with typhoid on August 27. But in none of these cases was the certainty or probability of the dairy milk having been taken excluded. It cannot be asserted with certainty that they got their fever from a contagium evolved into the atmosphere at the cowyard. In the first instance the boy lived on the business premises, and had his meals there, including breakfast and tea. In the second case the same observatian holds good. In the third case the girl had certainly taken the milk habitually up to a period within that of the incubation of typhoid. In the fourth instance the man told me that he occasionally took his meals at the coffee house, or his tea at his mother's house, both supplied from the dairy. In the fifth case the young man habitually took his meals at the above-named coffee shop, and also sometimes carried out the milk. The sixth case alone can be supposed to have probably originated in the effluvia of the yard. This was the cowman, who boasted that he always got his milk pure, for that he drank half a pint every morning early while milking the cows, and that what he used at home with his family was taken from the cowhouse direct in a bottle. I may observe here that in this case the disease was confined to himself, no one else of his family or in the house getting typhoid. But, then, as he also carried out milk from the dairy he had the opportunity of quenching his thirst from the cans, whether he availed himself of it or not. He says he never drank any water on either of the premises, as "he does not like water." Throughout my inquiry I have met with no other case of typhoid among men engaged at the yard than those I have mentioned. It appears, therefore, to me that the evidence of typhoid emanations into the atmosphere of the yard fails to be satisfactory.

This being so, we fall back upon the business premises. Here I found two possible sources of typhoid contagium—viz., the offensive emanations from the stable drains and the underground water-tank which supplied the pump. The evidence of

mischief from the former is as deficient as it was in the case of the emanations in the cowyard; none of the men exposed to them having had fever except those who had been in the way of drinking the milk. And there is this additional fact, that the family most constantly and thoroughly exposed to these emanations escaped—namely, that of the horse-keeper, who actually lived over the stable, and the staircase leading to whose rooms was erected over a closet (not supplied with water) used by the workmen on the premises.

It will be observed that, step by step, my inquiry has become narrowed by the process of exclusion—the only one as it appears to me, applicable in such an investigation as this. And so I have arrived at the *underground tank*. About this for some weeks I could learn nothing but that it was a wooden tank, constructed sixteen years ago, of three-inch pine bound with iron, and puddled at the bottom with about nine inches of clay. Awaiting the time when the tank could be opened for inspection, I forwarded samples of the water for analysis to Dr. Bernays. I did not expect much to come of this, since the outbreak was then nearly over, and an analysis made in September of water which was daily undergoing replenishment would probably give very different results from an analysis made in June or July, when it might have been somehow contaminated; and the result of the analysis confirmed my expectation. It was in all respects similar to the results of analysis of the New River Company's water, as published by the Registrar-General and by Dr. Letheby—that is to say, it exhibited no special character that would indicate an unusual departure from the average purity of that water.

It was not until the return of the family from the country that I could get the tank exposed; and what found was this:—The description given me of the tank was correct; but the woodwork had become rotten, and at one corner towards the cart-shed, had broken down to the depth, from the edge, of about eight to ten inches, forming a considerable gap in that side of the tank. (c) Between the part of the tank where this gap existed and the adjoining wall (distant about twelve inches), the earth, present everywhere else between them, was wanting, a conical hole being formed to the depth of about two feet. At the bottom of this hole, on letting down a candle, I noticed a rounded, smooth shoot with a wet clay bottom, inclining towards the cart-shed, and, on introducing my hand, I was convinced that it was a rat burrow. A stone was raised in the direction of the cart-shed, a hole dug, and the burrow was again reached, and found to cross above a stoneware pipe leading from the water-closet towards the stable. Fortunately, at this moment the water was turned on from the main. It

rapidly filled the tank, which overflowed at the gap; the water ran into the hole dug outside, and filled it to the level of about four inches above the burrow. It then ceased to rise any more, and on carrying my hand round this level, I found another burrow, by which, evidently, the water was running off. On the flow by the supply-pipe ceasing, the water sank rapidly to the level of the lower burrow, and was seen running away by that also. It sank no lower for a long time. The inference I drew was that by these burrows the water was very freely conducted away into some drain or cesspool. On taking up the paving of the yard and exploring, three old and imperfect brick drains of various sizes were discovered, into one of which the pipe from the water-closet had been carried. The other two were disused, but one of them contained some wet slush; the other, containing only a quantity of dry deposit, could not have been the conduit for the overflowing water. In making these excavations, the burrows were necessarily trodden in, but the soil was found saturated with wet up to the brick drain in the stable that the water-closet pipe ran into, and up to the old drain at a lower level that contained the wet slush. With the exception of the drain which received the water-closet pipe drain, these old drains were disused, and evidently had been disused for many years. I have marked the situation of all three upon the plan. From the appearance of the soil along the dotted lines, there is every reason to believe that it was in their direction, or by some diverging burrows, that the water ran away into the two drains marked A and B. With respect to the old drain A, it is believed that it was once an overflow drain running into an old cesspool or ditch which is known to have once existed outside these premises, and the remains of which, beneath a collection of old oyster-shells, were discovered about June last in laying down a pipe-drain, D, for the drainage of some neighbouring houses. Prior to the laying down of this drain, the stable brick drain, B, ran straight along into an old sewer; but when, D, a six-inch pipe was laid in, it was thought right to connect the brick drain, B, with it, the owner intending at some future time to do away with B, and carry the pipe from the water-closet into D.

This being the condition of affairs, it is evident that the underground tank was, by these rat-burrows, in communication with two old drains; and it is scarcely necessary to point out that, where rats could pass and water rapidly flow away, foul gases from the drains could pass, and must have passed, to the tank. Moreover, any accidental or temporary arrest of the flow of sewage from the brick drain B,—such for instance, as may have occurred at the time of the alterations of the drainage of the adjoining houses—must, at the time the water flowed

from the main into the tank, have filled up the drains and rat-burrows and the hole between the tank and the wall, and have caused an overflow of sewage into the tank at the gap. Either such an accident or the completion of the burrows to the tank, or the incursion of a rat carrying foul matters with him, might serve to account for the suddenness of the outbreak, supposing this water to have been used habitually for addition to the milk. However this may have been, we have here a fouled water-tank, the addition of the contents of which to milk distributed in the neighbourhood would be sufficient to account for the communication of typhoid fever to those who used it.

Is there, then, any reason to believe that this water was ever added to the milk, or that the water alone ever produced typhoid? I was assured by the men about the premises that none of them ever drank this water; it was understood generally on the establishment that it was provided for the horses, for washing the cans, and for cleaning purposes generally. I only met with one boy who drank of it, and he used it at his dinner, and whenever he was thirsty. He had fever; but then at that time he was living upon the premises, and had his meals there, with the usual quantity of milk at his breakfast and tea. He was there for a week at the beginning of August, and was taken ill on or about August 14, after he had returned to his mother's house. This boy agreed with the other men in saying that he had never seen anybody else drink from the pump. The family of the deceased dairyman also informed me that at no time was the pump used for the addition of water to the milk; "if ever any was added, it was from the tap." Certainly the tap was the more conveniently situated for this purpose, close on the right side, and a little below the flap-table. So far as their knowledge extended, I have every reason to give credit to this statement, which, moreover, has been confirmed to me independently by several of the men employed. But then it was added that those of the family I inquired of had little or nothing to do with the milk business, which was entirely under the management of the young man who died; and it is to be recollected that persons in this trade are very much dependent upon the honesty of their servants, and that the pump, although in full view of the office when occupied, was not in full view of the window in the house which looked upon this part of the yard. And in connexion with the hypothesis that contaminated water had on some occasions been added to the milk by some one is a fact communicated to me that one family while suffering from typhoid discontinued the use of the milk "because it had a bad taste, and was disagreeable;" and another person asserts that "she had several times complained to the dairyman himself that the milk when kept became

stinking, not (as she said) merely sour, and also of its poorness" This, if true, must have been prior to July 9. We are dealing now with probabilities, and one point to be weighed in estimating them is the fact that the dairyman and his household were among the very first attacked with the fever. Did they use milk diluted with this water in their family? Probably they were not very particular. But, supposing that it is absolutely true that no water from the pump was purposely added by any one to the milk, we have yet left the admitted fact that the cans were washed at the pump. Is it probable that the small quantity of foul water left in them after this process would suffice to contaminate the whole bulk of milk subsequently introduced? I confess that, to my mind, this is not an impossibility. We all know how small, almost infinitesimal, an admixture of sewage will poison a well or running stream; nor is the idea of reproduction of the typhoid contagium out of, within, or in the presence of an appropriate organic material at all foreign to the prevailing opinions upon the subject. Future experience may show that milk, which has remarkable relations to chemical ferments, is a substance peculiarly adapted also to the reproduction of morbid contagia, or to the contagium of typhoid in particular. Nor do we know even now how minute a quantity of contagium is sufficient to introduce the disease into any individual. Scarlet fever has recently been shown by Dr. Bell, of St. Andrews, to have been conveyed by milk to the customers of a cowkeeper in that place, the cows having been milked by persons convalescent from the disease. I claim now to have shown, what I have long suspected to be probable, that typhoid fever may be similarly conveyed by milk. Whether I have also correctly referred the contamination of the milk in this case to its source in contaminated water, others are better judges than myself. At all events, I have spared myself no labour in the investigation, which, if it has resulted in nothing else, has demonstrated one of the dangers connected with the mode in which the trade in milk is conducted in this metropolis.—*Medical Times and Gazette, Nov.* 26, 1870, *p.* 612.

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## 6.—THE ADMINISTRATION OF SULPHUROUS ACID IN TYPHOID FEVER.

By Dr. G. WILKS, Ashford, Kent.

[In the summer of 1869 a great number of cases of typhoid fever occurred in Ashford, at first sporadically, and after a time in greater numbers. The diarrhœa was profuse, and exhausted the patients so much that it was absolutely necessary to control it. For this purpose the ordinary astringents in use completely

failed, and Dr. Wilks tried creosote. This seemed to be beneficial, but nauseated so much that it had to be given up, and one patient, the only fatal case, attributed his approaching death to this drug.]

The first patient upon whom I tried it was a daughter of the gentleman who had attributed his death to the creosote. She was very ill before I saw her—alarmingly prostrate both in body and mind, vomiting and purging, with the abdomen tympanitic and painful to the touch, and distinct gurgling about the ileo-cæcal valve on palpation. The pulse was 120, and very feeble; the temperature 103 deg. With some fear as to the issue of the case, I began to administer the sulphurous acid freely. After twelve hours, I saw her again. The vomiting had ceased; the purging was fast abating; the tympanitis was much reduced; the feeble pulse had *not* grown feebler; but the parched tongue was moister, and the thirst less. She said that the medicine, each time she took it, relieved her within a few minutes of the indescribably miserable feeling in her bowels, and of the nausea which had before oppressed her. She could now take the nourishment ordered without fear of retching; and the whole aspect of the case, from the first a severe one, was more hopeful. To cut a long story short, she recovered, though she had entirely resigned all hopes.

Plentiful opportunities for using the acid came to hand; and the more we tried it, the better we liked it, and the more firmly we became convinced of its efficacy. This summer we have used the acid in more than a hundred and seventy cases with signal results; of them all, one only died, and he was an habitual drunkard, and would not take his medicine. In one case only did we withhold the acid; the patient, a female, died. But the circumstances were exceptional, and probably more to blame for the death than the want of sulphurous acid was.

I will now quote from my note-book a few of the more remarkable cases.

*Case* 1.—Mary W., aged 73, almost a pauper, had been ill a fortnight, and confined to bed three days. She had vomiting and purging, with yellow blood-streaked fluid stools. The abdomen was tympanitic. She was unconscious, save when strongly roused. Pulse 130, fluttering; respiration 30. The tongue was white at the edges, brown in the centre, and dry as a chip. The diagnosis was easy, for the rash was developed. The prognosis was very unfavourable; but she took the acid, and has since married.

*Case* 2.—Annie P., aged 4, was the child of wretchedly poor parents. Her mother, far advanced in pregnancy, was herself suffering from the fever in a mild form. This child had been ill in bed for ten days, and was, it was thought, moribund when

I was sent for. She was continuously delirious, purging incessantly, so that the whole house stank of her offensive motions. The rash was copious; the pulse uncountable; the tympanitis extreme; the vomiting so persistent that the attendants had for hours ceased to give her food. Could any case in itself, and in all its collateral circumstances, be more unpromising? With the utmost difficulty, and by dint of the sternest resolution, I forced the grandmother, who nursed the two patients, to give the child the medicine regularly, and feed it every ten minutes. For four days it lay seemingly at the very point of death, so that every time I entered the house I was taunted with harassing the last moments of a dying child. She took the medicine, and recovered.

*Case* 3.—Annie F., aged 2, an only child of poor but not poverty-stricken parents, had all the above mentioned symptoms. You will doubtless agree with me that an only child is a more troublesome patient than one of a more numerous family; yet this child too recovered under sulphurous acid treatment.

*Case* 4.—Edith M., aged 3 months, the child of well-to-do tradespeople, did not have the rash, but the vomiting, purging, and all the other symptoms, as badly as any case; but she too recovered.

*Case* 5.—Thomas H., aged 54, a well-to-do mechanic, had been under the hands of a skilful medical man for some weeks with such symptoms as I have before described; but, so often as the diarrhœa was checked, the burning heat and vomiting were greatly increased, and *vice versa*, till at last he was told he could not live the night, and was beyond human help. He then sent for me. When I saw him, I found him in a state of most alarming prostration. His pulse was too fast and feeble to be counted. He had vomiting; the abdomen was tympanitic; and he had purging, the bowels having acted thirty-four times in the last twenty hours. I could not discover any rash; but his state was such as to preclude any very searching examination. I prescribed the acid. In the next sixteen hours his bowels acted twice; he was not once sick. He recovered. This case seems to me to say volumes for this medicine, for there is no doubt that the man had taken all the drugs ordinarily used without deriving permanent benefit or arresting the disease; and yet from the moment he swallowed the first dose of sulphurous acid he began a recovery in which he never faltered afterwards.

The acid should be given in doses of from two and a half to twenty minims, according to age, repeated every four hours, and continued for a week, ten days, or even more, until the patient complains of tasting, smelling, or feeling like sulphur

or lucifer matches; or, in the case of infants, until they actually emit an odour of the gas from their skin and breath. I have never pressed its use beyond this, under the belief that the system must by that time be supersaturated; nor have I ever seen reason to regret stopping it at that point.

Where I have seen the case early, before the diarrhœa has become severe, I have given simply the sulphurous acid flavoured with syrup of orange-peel in water. Where the diarrhœa was troublesome, I have added sulphuric acid and laudanum, according to the age of the patients. Thus my usual formulæ for adults have been the following.

℞. Acidi sulphurosi ʒ ij; syrupi aurantii ʒ iij; aquam ad ʒ vj; or

℞. Acidi sulphurosi ʒ ij; acidi sulphurici diluti ʒ ij; tincturæ opii ♏ xx; syrupi aurantii ʒ iij; aquam ad ℥ vj.

For infants:

℞. Acidi sulphurosi ♏ xv; syrupi aurantii ʒ iij; aquam ad ℥ j; or

℞. Acidi sulphurosi ♏ xv; acidi sulphurici diluti ♏ xv; tincturæ opii ♏ ij; syrupi aurantii ʒ iv; aquam ad ℥ j.

A sixth part of either of these mixtures is to be taken every four hours.

In one case only can I recollect diarrhœa *setting in* during the sulphurous acid treatment, and without having previously existed; but the patient had old standing renal disease, from which she still suffers, though she has almost forgotten fever.

Lest I should be misunderstood, I will state distinctly what I claim for the sulphurous acid in typhoid fever: that it arrests the further development of the fever-poison, and, by continuing this arrest long enough, exterminates the fever. Briefly, it is an antidote.

In some of my early cases, I left off the acid after a few days' use, because the patients seemed better. In almost all such cases, they had a relapse, which was again immediately arrested by the resumption of the acid.

I believe that, where we get the cases soon enough, we may stamp out the disease then and there. For instance, in one thirty-six hours I was sent for to see four children belonging to two different families visiting in the town. I had no doubt as to what their symptoms denoted, and at once gave the acid. At my visit next morning, I was met in each house with smiling faces and the assurance that I had been mistaken in my diagnosis, for the little patients were much better. Of these children, three remained under my care, took the acid for a week, and made complete recoveries, without relapse. The fourth seemed to ail so little that the parents would not heed my advice, but carried her home after two days of the acid.

Shortly afterwards, I learned that, three days after reaching home, she had been attacked by typhoid fever, and hardly escaped with life. I make no doubt that, had the other three also discontinued the acid so soon as they seemed well, they too would have had a return of all their symptoms, and have suffered the ordinary course of typhoid fever.

Very possibly I have failed to convince you of the advantages of using this drug; but let me again remind you of the plain fact that, of one hundred and seventy-three cases of this fever occurring in our practice during the past fourteen months, two only died, and those two did not take the acid: for the one it was not prescribed; the other was a drunkard, and would take nothing. Of the hundred and seventy-one who took sulphurous acid, not one lost his life; and there were but few who were not convalescent within fifteen days of commencing the treatment. Surely such a result will induce you to try the medicine for yourselves when opportunity offers.—*British Medical Journal*, *Dec.* 3, 1870, *p.* 599.

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## 7.—REPORT ON DISINFECTION WITH EARTH, &c.—EARTH CLOSETS.

The disinfecting power of earth has been known from the earliest time; in fact, the burial of the dead and the putting of unclean things into the ground are examples of its use from the remotest antiquity. In the ordonnances of Moses, which were written for the guidance of the Israelites, specific instructions are given for the disposal of the blood of slaughtered animals, by pouring it upon the ground, and of "that which cometh from thee," by putting it into the earth and covering it therewith, so that the camp may be clean and holy; and again, the custom in China of mixing earth or fat marl with human excrements, and so forming them into a portable and odourless manure is of very ancient date. It is somewhat surprising, therefore, that among all the inventions in this country for the disposal of human excreta, there have been none, until recently, for the disinfection and consolidation of them by means of common earth. Rosser, in 1837, obtained a patent for the treatment of fæcal substances, urine, and house refuse with herbaceous plants and common earth, to which were added unslaked lime, soot, powdered gypsum, &c. Twenty years later, the Rev. W. R. Bowditch, one of the vicars of Wakefield, who had been studying the absorbent action of clay in the impurities of coal-gas, recommended, in the *Journal of the Agricultural Society* for 1858, that dry pounded clay should be put into a chamber or cistern above the closets of houses, and distributed over the fæces, &c., by means of some mechanical contrivance,

to be worked by a handle, there being a water-tight box below, which could be moved away and emptied when necessary. About the same time the Rev. H. Moule, who is the vicar of Fordington, in Dorsetshire, had practically tested the deodorising power of earth on privy soil; for having abolished the cesspools of his house, on account of their unwholesome and offensive nature, he substituted small buckets, which were placed beneath the privy seats, and these were emptied daily into a trench in his garden, where the foul matters were immediately covered with earth. He soon noticed that when the trench was reopened the matters from the closet had not only lost their offensive odour, but had actually disappeared by a process of disintegration. This led him to the experiment of putting earth into the buckets, and of drying the contents in a covered shed. After working in this manner with about a load of earth, and redrying it, he found it was so perfectly inoffensive that he could use it over and over again, at least a dozen times in succession, and thus he produced a valuable manure, containing about one-third of its weight of dried excrement. The next part of his inquiry was devoted to the mechanical process of supplying the dry earth to the closet instead of water; and in the month of May, 1860, he obtained a patent, in conjunction with Mr. James Bannehr, for "improvements in the nature and construction of closets and commodes for the reception and removal of excrementitious and other offensive matter, and in the manufacture of manure therefrom"—his claim being for the use of dry earth, clay, loam, or peat, powdered and sifted, and applied to the excrementitious matter, by means of certain mechanical contrivances, so as to substitute a dry closet or commode for a water-closet, the earth, &c., being repeatedly used in this manner after it had been properly dried. Since that time a number of patents have been taken out for improvements in the machinery of the apparatus, the best form of it being that which is used by the Earth-closet Company and patented by their engineer, Mr. Girdlestone.

Experience has shown that the quality and condition of the earth employed in the closets have much to do with the success of the process, for pure or nearly pure sand has little deodorising power; and the same is the case with chalk, and other forms of carbonate of lime. Peat also, although rather more effective than sand or chalk, is not a good deodoriser; whereas clay, or earth which is rich in clay, is well suited for the purpose. The degree of dryness is also a matter of considerable importance, as it seriously affects the absorbent power of the material —hence the necessity for selecting a heavy soil, like brick-earth, which is loaded with clay, and then drying it by artificial means, in order that it may be powdered and sifted for the purpose of utilizing its absorbent action to the fullest extent.

As to the quantity of earth required on each occasion, it appears, from observation and experiment, that a pound and a half of dry earth is sufficient for each solid evacuation (amounting to from four to five ounces in weight), and the same quantity for each liquid discharge (amounting to six fluid ounces in bulk). Practically, indeed, it is found that 4½ lbs. of dry earth per head per day is sufficient for all purposes, and in illustration of this a few working examples may be given. In the Dorset County Gaol at Dorchester, where the inmates are all adults, three pounds of earth are used per head daily, but the product is wet and offensive, showing that the quantity of earth is not enough for the proper consolidation and deodorisation of the excreta; whereas, at the Dorset County School in the same town, with eighty-three boys, a ton of artificially dried earth is used in the four closets weekly. This is at the rate of 4 lbs. per head daily, and it forms a solid inoffensive compost. At the villages of Halton and Aston Clinton, on the estate of Baron Rothschild, near Wendover, in Buckinghamshire, there are fifty-five well-managed earth closets, of good construction. These accommodate about 300 people, and during the last four years they have been supplied with dry earth at the rate of 130 tons a year, which is in the proportion of rather less than 2¾ lbs. per head daily, but the closets do not receive the whole of the day urine. In Lancaster also, where there are 90 earth latrines with 200 seats, accommodating 2250 persons, belonging to 450 houses, the quantity of earth used is 14 tons a week, which is a little less than 2 lbs. per head a day; but, as in the last case, a good deal of the urine is not discharged into the latrines. Again, at the volunteer camp at Wimbledon, the experience of the earth system is particularly instructive, for there are 114 latrines fitted up with earth closets in the most complete manner, and 41 urinals with earth-pits for the absorption of the urine. All the contrivances have been arranged by Mr. Girdlestone, the engineer of the Earth-closet Company, and they have been kept in good working order. Dr. Buchanan has ascertained, from careful inquiry at the camp, that during the meeting of the volunteers, which lasts for fourteen days, as many as 3000 persons use the closets daily, and 10,000 the urinals. In the course of that time 140 tons of dry earth are expended in the closets and urinals, the closets requiring 4500 lbs. per day, and the urinals 17,900 lbs. This is in the proportion of 1·5 lb. of earth for each operation at the closets, and 1·79 lb. for each visit to the urinals. When the earth was dry and of good quality it was found that the product was solid and inoffensive; but when, as in the preceding year (1867), it was of a peaty nature, the compost was wet and sour. Taking these facts into consideration, Dr. Buchanan concludes

that 4½ lbs. of dry earth would be a proper supply for each person daily, 1½ lbs. being allowed for each visit to the closet or urinal, and three such visits daily. A village of 1000 inhabitants would, therefore, require 4500 lbs., or just two tons of dry earth per diem.

The method of using the earth will necessarily vary according to circumstances. At Lancaster, where the latrines are under the superintendance of the local authorities, the earth is thrown into the closets in one application daily, or it may be supplied by a scoop after each operation, as at the Dorset County Gaol; or, better still, by proper contrivances for ensuring a regulated delivery of earth, as in the patent earth-closet or commode of the company; and the foul earth may be removed from the pit or vessel at any convenient time up to two or three months. After its removal it may be again dried and returned to the closet, until it has become charged with materials of manurial value.

The importance of this arrangement in a sanitary point of view is considerable, especially in warm climates, where the desiccation of the earth is easily effected. This has been clearly established in India, where the system has been rapidly extended.

The cost of working the system has been carefully computed by Dr. Buchanan, who says that in a village of a thousand people it would be £260 a year, and that the value of the manure (say 720 tons at 10s. per ton) would be £360, supposing that the earth had been used only once; but if employed four times over, the annual outlay would be reduced to £244, and the product, amounting to 200 tons a year, would, at £3 a ton, the price at Dorchester, and the estimated value of it by Mr. James, the agent of Baron Rothschild, realise £600 a year. Even at the approximate value of 10s. per head of the population, it would be worth £500; and this, he thinks, would be a profitable return, helping to pay the cost of other sanitary work. In fact, the advantages of the system, as summarised by him, are as follows:—

"1. The earth closet, intelligently managed, furnishes a means of disposing of excrement without nuisance, and apparently without detriment to health.

"2. In communities, the earth-closet system requires to be managed by the authority of the place, and will pay at least the expenses of its management.

"3. In the poorer classes of houses, where supervision of any closet arrangements is indispensable, the adoption of the earth system offers especial advantages.

"4. The earth system of excrement removal does not supersede the necessity for an independent means of removing slops, rain water, and soil water.

"*5.* The limits of application of the earth system in the future cannot be stated. In existing towns, favourably arranged for access to the closets, the system might at once be applied to populations of 10,000 persons.

"6. Compared with the water-closet, the earth-closet has these advantages:—It is cheaper in original cost; it requires less repair; it is not injured by frost; it is not damaged by improper substances being thrown down it; and it very greatly reduces the quantity of water required by each household.

"7. As regards the application of excrement to the land, the advantages of the earth system are these:—The whole agricultural value of the excrement is retained; the resulting manure is in a state in which it can be kept, carried about, and applied to crops with facility; there is no need for restricting its use to any particular area, nor for using it at times when, agriculturally, it is worthless, and it can be applied with advantage to a great variety, if not all, crops and soils."

On the other hand, the chief objections to its use are—the notion of its filthiness as contrasted with the cleanliness of the water-closet, and the difficulty of supplying the necessary quantity of dry earth, and of removing the foul compost. The advocates, in fact, of the present water-closet system assert that water is a vehicle which will carry the filth, by the natural power of gravitation, to any place where it is wanted; and that it carries it more cleanly, more cheaply, and more immediately than can be done by any organisation of man and horse and cart. "The manure which must be collected from town privies by an army of scavengers, and distributed from the depôt by barge and rail, to be afterwards loaded in carts and spread abroad by hand, and covered by the plough, might, if water were the carrier, be virtually self-borne to the very place where it is wanted,—taken almost direct from the water-closet to the field, and there washed in at once with really no labour at all among the very roots of the plants it is to feed." But all this assumes the total absence of inconvenience and danger from misadventure of the soil between the closet and the field, and it assumes, moreover, that the ground is always ready to receive it and the growing crop to appropriate it,—that there will, in fact, be no escape of noxious effluvium into the air, or of foul matters into the earth or into the neighbouring water-courses. Experience, however, has shown that these assumptions are never realised in practice, and that the greatest difficulties of the water-closet system are created by the very circumstances which are thus assumed to be non-existent,—that the vehicle itself, which is said to be so admirable a servant, is really a

tyrannical master, defying all our efforts to control it; and hence there are many large towns already sewered which will not accept this vehicle notwithstanding its plausible show of convenience.—*Medical Press and Circular, Feb.* 15, 1871, *p.* 131.

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## 8.—AN ANALYSIS OF 800 CASES OF SMALL-POX, OBSERVED DURING THE PRESENT EPIDEMIC IN THE HAMPSTEAD SMALL-POX HOSPITAL.

By Dr. GRIEVE, Medical Director to the Hospital.

[The 800 cases were admitted between the 1st of December, 1870, and the 18th of February, 1871. It must be borne in mind that the cases were selected from amongst a number of milder cases, and that the mortality was consequently rather high.]

As to vaccination. Of the 800, 591 were vaccinated and 209 were not. Amongst the vaccinated are included only those upon whose arms some kind of marks could be distinguished. And this large ratio of unvaccinated cases may help to account for the high general mortality. When we come to examine the relative proportions of vaccinated to unvaccinated admitted at different ages, the result tends to shake one's belief in the commonly accepted doctrine that liability to small-pox decreases with advancing age, While under ten years of age the unvaccinated admitted are to the vaccinated as 2 to 1, between ten and twenty the proportion is more than reversed, and the vaccinated are as 4 to 1 to the unvaccinated, which becomes 5 to 1 between twenty and forty, and 9 to 1 over forty years of age. The number of unvaccinated shows a regular diminution in advancing periods: below ten there were 103 admitted; between ten and twenty, 46; between twenty and forty, 56; over forty, 4. I believe that very few unvaccinated people reach forty without having had an attack of small-pox, and that this accounts for the diminished mortality amongst adults to the number living; and this view is borne out by an account published the other day of the results of a visitation in search of unvaccinated persons in St. George's-in-the-East. The report says: "The numbers of persons discovered unvaccinated in the course of the visitation were as follows: 589 under three years of age, 168 between three and fourteen years of age, and 10 adults." From this it would seem either that vaccination has been much more neglected during the last three years than in the preceding eleven; or, more likely, the last epidemic of small-pox has had something to do with thinning the ranks of the unprotected.

If in the census which is to be taken soon the fact of being vaccinated or unvaccinated were stated concerning each individual, the result would be a large amount of valuable information. But a good deal might be done towards elucidating this point by medical men who are in the habit of examining recruits for the army or police force. It is needless for me, I hope, in an assembly chiefly composed of medical men, to express my firm conviction and belief in the powerful effect exercised by vaccination over subsequent small-pox. I conceive its benefit is shown in three ways: it lessens the liability to take small-pox; it reduces the consequent mortality; and it modifies it even when it does occur, so as to change it from one of the most loathsome and fatal of all the zymotic diseases (as assuredly it was before Jenner's discovery) into a comparatively mild and harmless attack. And even when a fatal result ensues in a vaccinated case, as certainly frequently happens, the cause lies as much in some constitutional deficiency as in the disease itself, and the outward manifestations are never as severe as in the unvaccinated. One very marked difference is in the size of the pustule, which, I may say, is almost invariable. In the unvaccinated you have a large, round, flattened pustule; in the vaccinated you have a smaller acuminated one. These figures prove the foregoing assertions as to mortality: of 591 vaccinated cases, 58 have died, being a percentage of 9·8; of the 209 unvaccinated, 96 have succumbed, a percentage of 45·8. The difference is perceptible enough. Now, as to severity of the disease. In vaccinated cases it has been found necessary to detain on an average twenty-four days in the hospital; unvaccinated have to be kept thirty-five days.

Does the effect of vaccination wear off? The Registrar-General, acting upon the supposition the fallacy of which in one way I have shown before, answers this question in the negative; but the experience of these 800 cases teaches differently. He, in treating of this question, took the gross mortality, both vaccinated and unvaccinated, at different ages, and so made his deductions. The great fatality amongst the unvaccinated is in infants, and so the gross mortality in infants is raised; but take the vaccinated separately, and we find under ten years of age it is 9¼ per cent.; between ten and twenty, 2¼ per cent.; between twenty and forty, 12⅔ per cent.; and over forty, 22½ per cent. There is an apparent contradiction here, when we have a larger per centage under ten years of age than between ten and twenty; but all engaged in practice know that in very young infants, in all disorders, complications carry them off; and that really a great part of the mortality in these cases may be put down to bronchitis, teething, &c. From twenty onwards the percentage of deaths amongst the vaccinated increases steadily. To go safely to work, then, revaccination

ought to be performed somewhere about the age of puberty. The value of revaccination as a complete preventive against small-pox has been proved by Mr. Marson's experience at the Small-pox Hospital; and as far as ours goes at Hampstead it confirms it. We have sixty officials connected with the hospital, who are constantly in contact with the sick; and in the three months we have been open not a single case has occurred amongst them. A curious thing also is, that of the 800 cases under discussion to-night not one could prove that he or she had been revaccinated; while we had two suffering from a second attack of small-pox.

A noticeable feature in the class of people attacked is, that for a considerable time the number of men sent to the hospital by far outnumbered the women and children. Now we have many applications for the latter: the men, who, among the working classes, from their more active habits, are more likely to come casually in contact with the disease, have evidently taken it home, and from them their wives and families have been affected.

The number of our patients who are in occupations bringing them in contact with the public, such as barmaids, shop men and women, 'bus- and cab-drivers, &c., shows that, to prevent a contagious disease from spreading, nothing less than compulsory isolation while the contagious power remains is sufficient.

There are other points connected with these cases which I may have an opportunity of bringing before your notice when the epidemic is over, and more leisure to give to their notation than there is at present. I will read the numbers admitted, at the different ages, of vaccinated and unvaccinated, and the mortality and percentage during those periods:—

There were 800 cases admitted between the 1st December, 1870, and the 18th February, 1871.

| | No. Admitted. | Deaths. | Percentage. |
|---|---|---|---|
| Under 10 years, vaccinated .. | 51 | 5 | 9·8 |
| Ditto unvaccinated.. | 103 | 54 | 52·4 |
| Between 10 and 20, vaccinated | 211 | 6 | 2·8 |
| Ditto unvaccinated | 46 | 13 | 28·2 |
| Between 20 and 40, vaccinated | 289 | 38 | 12·7 |
| Ditto unvaccinated | 56 | 26 | 46·42 |
| Over 40, vaccinated .. | 40 | 9 | 22·5 |
| Ditto unvaccinated .. | 4 | 3 | 75 |
| | 800 | 154 | 19·25 |
| Total vaccinated .. | 591 | 58 | 9·8 |
| ,, unvaccinated .. | 209 | 96 | 45·8 |

—*Lancet, March* 18, 1871, *p.* 371.

## 9.—ON THE TREATMENT OF BLOOD-POISONING BY A CARBOLIZED ATMOSPHERE ACTING THROUGH THE SKIN AND LUNGS.

By JOHN WOOD, Esq., Surgeon to King's College Hospital.

[For some time back Mr. Wood has used at King's College Hospital, in cases of pyæmic and erysipelatous blood-poisoning, and also as a prophylactic agent after operations, the vapour of carbolic acid evolved constantly under and retained by the bed-coverings. The vapour thus remains sufficiently long in contact with the patient's body for absorption to take place into the system.]

For the purpose indicated, I employ the powder known as Macdougall's disinfecting powder. This is placed in small muslin bags to a considerable extent pervious to the dust, and wholly so to the vapour which is emitted. These are suspended from the ribs of the cradle, which is almost universally used to keep off the weight of the bed-clothes or to swing the limb, in surgical cases. Where these are not used, the small bags placed on the bed near and around the wound are quite as effective. I have never yet found in any case the presence of the dust to be complained of as irritating to the skin of the patient, and I have used them, as the subjoined cases will show, with the best results in severe cases of traumatic erysipelas. I also employ the powder, freely spread under and around the bed, in all putrefying and infectious cases, and the floors are washed with a solution of the common carbolic acid, so as to pervade the atmosphere very sensibly.

As regards the effect upon the wound itself, it seems to do all that is ordinarily necessary to keep the parts totally free from putrescence or smell of an offensive kind, when large sloughs are not actually present, and the wound very putrescent. When such are present I have usually used, in addition, the carbolic oil, of the strength of one part in six or eight, as a direct application, so as to saturate the sloughing part. In doing so, care is taken to avoid contact of the oil with the surrounding skin or with healthy granulations, upon which it has invariably the effect of retarding development and cicatrization. The perfect freedom of the beds, even in the worst cases, from smell and offensiveness is very remarkable, and this alone cannot but be beneficial to the patient's condition, and adds to his power of resisting septic influences.

In one case of undoubted pyæmic infection after operation, the effect of the absorption of the acid was evident both in the breath and in the urine. The latter presented for the space of a week a characteristic slate-coloured film and deposit. Some of this was collected and analysed, and was stated to be a

modification of the colouring matter of the urine, and to be, in fact, identical with the ordinary blue indigo, and probably formed by transformation of the yellow indigo of the excretion. Since that time I have observed that a similar sediment has been observed in other cases subjected to the action of carbolic acid.

In the adjoined cases this appearance has not been observed in the urine. I have selected them as being fair specimens of a recent accession of erysipelatous and pyæmic cases in our hospital; and as having had the advantage of careful temperature observations made by Messrs. Whitmore and Rope, the house-surgeons; and also as having the carbolic vapour treatment perseveringly carried out throughout.

The two cases of *erysipelas* were as severe as any which I have ever seen recover. The stimulating plan was fully carried out, and due justice must be rendered to it and to the administration of the tincture of the perchloride of iron, although the latter did not form a principal part of the plan of treatment. The carbolic vapour did not seem to irritate the erysipelatous inflammation, which pursued in every respect its usual course of spreading, vesication, and consecutive abscess. The advantages which this method of application afforded in these cases were to leave the stomach free to the other remedies, to permit a complete freedom of escape of the pus and sloughs from the wounds, and their treatment by more soothing applications; while, at the same time, the benefits of the carbolic antiseptic action were fully obtained, as proved by the complete absence of putrefaction or any unpleasant smell whatever throughout the whole progress of the cases.

The case of *pyœmia* is remarkable for the distinctly marked and well-connected history of a previous threatening of a similar attack after a wound of the foot, probably resulting in some damage to the knee-joint at that time; and also for the *complete and rapid recovery* of the patient, with a stiff knee-joint, after the *total necrosis and removal of the patella* through a free opening for the evacuation of the pyæmic abscess of the joint. I have not been able, up to the present time, to discover a record of such an occurrence, nor to find it in the experience or remembrance of any of my pathological friends.—*Practitioner, Jan.*, 1871, *p.* 1.

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## 10.—OBSERVATIONS ON THE INFLUENCE OF EPIDEMICS OF FEVER IN CHECKING THE ADVANCE OF THOSE OF CHOLERA.

By Robert Lawson, Esq., Inspector-General of Hospitals.

Though much has been said and written in support of different opinions as to the means by which epidemics of cholera

were diffused, comparatively little attention has been paid to the courses these pursued, or to the causes which seem to have influenced their progress from time to time, and to have ultimately turned each into the particular track which it followed. It is obvious that, as the acquaintance with the natural history of the disease increases, the application of the theories so eagerly supported at present by many epidemiologists will be narrowed more and more, and a point will ultimately be reached where, with a knowledge of causes as yet little heeded, rational explanations may be given of much that now seems unaccountable. The object of this communication is to contribute to the attainment of so desirable an end, by directing attention to a characteristic feature in the relations of fever to cholera.

While investigating the course of pandemic waves, I became aware of the influence epidemics of fever exercise in checking the advance of those of cholera, and *vice versa*. Fever has frequently prevailed over an extensive area, and though cholera, also embracing a large area, approached the other, yet, while the fever continued, epidemic cholera, as an epidemic, has never penetrated the fever field. Sporadic cases of cholera have frequently been met with a long way within the boundary of the fever field, and similarly cases of fever within that of cholera, but still the fact remains that, though the fever and cholera fields approached each other, neither disease took the place of the other until its force as an epidemic was broken. This fact sanctions the inference that the conditions which generate fever epidemics are not only different from those which produce epidemics of cholera, but are also incompatible with them; and further, that sometimes the one set of conditions, sometimes the other, exists over a large area of the earth's surface, and that the one will give way to the other without any marked change in the habits or circumstances of the population these areas embrace. The importance, therefore, of rightly estimating the influence exercised by one of these diseases over the diffusion of the other is obvious.

In the following relation, I have given, briefly, the course of cholera epidemics from India northwards, from 1818, and have placed in a parallel column the epidemics of febrile disease which were met with in their vicinity about the same time. The information available on these points is but too often fragmentary and incomplete, and does not show in all cases, the close limitation of the cholera fields by those occupied by fever, but, as far as the evidence goes, it bears out the general fact in every instance, that where fever prevailed there cholera did not penetrate. In particularising the various epidemics mentioned below, I have employed the principles detailed in the Papers on Pandemic Waves in the Sanitary Reports for the Army for 1864

and 1866, and refer the reader to the map accompanying the latter communication for further information thereon:—

In 1817, 1819, and 1821, fever was particularly severe in Cutch and Guzerat, and northwards towards Rajpootana; it was frequent also in the intermediate years. There was much fever in the Mediterranean stations in 1818 and 1819, plague in the former year in Morocco, and in the latter at Constantinople. Whether Persia and Asia Minor were embraced in this influence, which was manifest at Cutch to the south-east, and Constantinople to the north-west, I have no evidence; but as the febrific influence was then strongly developed throughout the Mediterranean, it most likely embraced the countries between these points as well. Be that as it may, however, epidemic cholera failed to penetrate this district.

In 1820 and 1821, fever was still prevalent in Cutch and Guzerat. In 1821, it was very severe among the troops in Ionian Islands, and continued in the western part of Mediterranean, but I have no notice of its prevalence in Egypt, Syria, or Asia Minor. In 1822, there was a great increase of fever among the troops at Malta. In the Ionian islands fever was less severe than in 1821, but still high.

In 1818, a cholerific wave passed the isoclinal, 30° N., and there was a severe epidemic of cholera in Northern India, which continued into 1819, but it did not extend across, or even so far as the Indus, in either year. In 1819 this wave would pass the isoclinal 53° N.; there is no further information, however, regarding the prevalence of cholera in its epidemic form; but in the Mediterranean military stations, in 1819, and more especially in 1820, there were indications of its influence in the form of common, or sporadic cholera.

In 1820, another cholerific wave passed the isoclinal, 30° N., and the disease, in its epidemic form prevailed extensively in Northern India in China, and passed the Indus into Beloochistan. It continued very active in Northern India in 1821, and spread along the Persian Gulf, and through Persia, as far as Yezd and Bagdad, or to about 34° N. In 1822 the epidemic resumed its activity, and extended to Diarbekir in lat. 38°, and approached the Mediterranean as far as Aintab and Aleppo. This wave would pass the isoclinal 53° N., in 1821, but, although cholera did not become epidemic anywhere in the zone to the north of it,

this or the following year, the cholerific influence was still very manifest in the Mediterranean military stations.

In 1823, the causes of fever were not active at either Gibraltar or Malta, but in the Ionian Islands the deaths from it among the troops were 40 per cent. higher than in 1822, the first indication in this neighbourhood of a severe and extensive outbreak of febrile disease, met with this year at Sierra Leone and Ascension, and which, in 1824, was manifested in greatly augmented ratios of mortality from fever at Gibraltar and Malta, by very severe plague in Egypt, plague in the Morea, Monte Negro, at Constantinople and Erzeroum; thus completely encircling the district in which epidemic cholera had shown itself, though the milder forms were still experienced far beyond it.

In 1822, another cholerific wave entered the zone between the isoclinals 30° and 50° N. This year the epidemic was not quite so active in Northern India as the previous one; it was severe in the north of Persia, apparently under the preceding wave, but early in 1823 it commenced there again, and, it is said, extended as far north as Astrachan and Orenburg in the course of the year; at the same time, it advanced from Aleppo westward, and reached Latakia and Antioch, and extended along the Mediterranean, between Scanderoon and Seleucia, where it seems to have stopped.

In 1826, there was a considerable increase in the mortality from fever among the troops in the Ionian Islands, which became still higher in the following year. In 1826, plague was severe at Constantinople, and in 1827 had included Odessa in its range. There was also much fever in Western Europe these two years.

In 1826, another cholerific wave passed the isoclinal 30° N., and the epidemic disease prevailed in Northern India and China. In 1827 this wave passed beyond the isoclinal 53° N., and the epidemic affected Tartary extensively, but did not advance westward into Europe, though the cholerific influence was still marked among the troops in the Mediterranean.

In 1828, febrile disease was particularly severe throughout the Mediterranean, and plague raged in Egypt, Syria, Greece, Constantinople, and in the Danubian Provinces, and continued to affect these countries,

In 1828, the succeeding cholerific wave passed the isoclinal 30° N., and cholera was very severe in Northern India. In 1829 this wave passed into the zone beyond the isoclinal 53° N., and in August the epidemic

and embraced Odessa the following year. In 1830, plague prevailed from Bassorah to Aleppo along the Euphrates, also in Egypt and Syria. The mortality among the troops in the Ionian Islands was as high as the previous year, and petechial typhus raged in the kingdom of Naples. There being no notices of the prevalence of fever over the western coasts of the Black Sea this year, it most probably had diminished considerably, coincident with the advance of cholera westward. I have no information as to the prevalence of fever through Europe in 1831, but, according to Muchison, typhus was prevalent in London that year, as also in Scotland, and the mortality among the Dragoons and Infantry of the Line, from fever, was much higher than usual.

In 1830, as mentioned above, plague prevailed in Egypt and Syria, and, consequently, though these countries are in the same zone as Northern India, cholera did not appear in them in 1830, the first year of this wave in the zone, but was delayed until 1831, the second year, when the plague had greatly diminished. The mortality from fever was considerably higher among the troops at Malta and Gibraltar in 1831 than the preceding or following years, though, in the Ionian Islands, it was little more than half what it was in 1830. The deaths from fever among the troops on the home station were all much reduced in 1832; this disease was less broke out in great force at Orenburg, and about the same time, at Tabriz, Tiflis, and in the northern parts of Persia, and it prevailed among the Tartar tribes to the north of the Persian frontier. In 1830, the second year of the wave in this zone, the epidemic extended westward of the Caspian and Volga, and embraced Russia as far as Moscow and Kazan, and in 1831, Poland, south shore of the Baltic, Hamburg, North of England, and even Iceland.

In 1830, when another cholerific wave passed into the zone between the isoclinals 30° and 53° N., there was no great activity of the disease in Northern India. In 1831, when the wave passed the isoclinal 53° N., cholera became epidemic in Egypt, and also appeared at Smyrna, Constantinople, and in Hungary. The following year, when the wave had entered the zone to the north of the isoclinal 70° N., the epidemic embraced England, Scotland, Ireland, and North of France, in Europe; and America, from the Gulf of Mexico to Canada.

prevalent among the population in London this year, too, though it was still active in Ireland and Scotland, and also among the troops in Canada.

In 1832, plague was prevalent at Bassora, Bagdad, Mecca, and other places in the Arabian Gulph. I have no information as to whether it was in Egypt. In 1833, there was a large increase of mortality, from fever, among the troops in the Ionian Islands, and a considerable rise at Malta, and in 1834, the ratio in the former was sustained, and that at Malta was again much increased, while plague was rife in Egypt, Tripoli, and at Constantinople. At Gibraltar, the deaths from fever were much diminished in 1834. I have no evidence regarding Morocco in either year. In 1833, there was a sensible increase in the mortality from fever among the troops in the West Indies, and in Jamaica, though diminishing, it was still high.

It was mentioned above that, in 1834, plague prevailed at Constantinople, Egypt, and on the coast of Barbary, as far as Tripoli, while fevers were common among the troops in the Ionian Islands and Malta. In 1835, plague continued very severe in Egypt. Pernicious fever was rife at Athens, though there was less fever at Gibraltar, Malta, and the Ionian Islands. In 1836, plague was very severe in Constantinople and through European Turkey, as far as the Danube. At both Malta and Gibraltar there was

In 1832, the following cholerific wave passed into the zone beyond the isoclinal 30° N. There was an increase of cholera in Northern India, which became still more frequent the following year. This wave passed into the zone beyond the isoclinal 53° in 1833, in which year Lisbon became affected, and Havanna, and Mexico, which is in the same zone as Northern India. In 1834, the second year of the wave, Spain was embraced in the epidemic, and it crossed the Straits of Gibraltar into Morocco. This year, too, beyond the isoclinal 70° N., Great Britain, Sweden and Norway, and north of Germany to the south of it, were included in the epidemic area, together with the United States, Canada, and Nova Scotia, and even Iceland.

In 1834, another wave entered the zone beyond the isoclinal 30° N. This year there was not much cholera in Northern India, but there was a considerable epidemic in the lower districts of Abyssinia, the second year of the wave there; whether it had shown itself the previous one, I have no information. In 1835, the wave passed the isoclinal 53° N. Cholera became epidemic in South of France, extending up the Rhine to Valence, and in Italy from the Alps as far south as Naples, and the following

a considerable increase of fever over the previous year. Fever increased in England and Ireland in 1836. In 1837, the plague continued in the Turkish Provinces along the Danube, but I have no notice as to whether fever was more frequent than usual to the northward. In England, Scotland, and Ireland there was, this year, a severe epidemic of fever, which extended into the following one.

In 1836, the Pali plague, which had been quiescent some years, reappeared in Marwar, and affected the country to the north-east, as far as Delhi. The following year, fevers in the northern part of India were particularly prevalent and severe. In 1836, the mortality among the troops in the West Indies from fever was greatly increased. In Jamaica, on the contrary, it was much reduced, though still high; but, in 1837, not only Sierra Leone, on the east of the Atlantic, but Demerera, on the west, and the West India Islands, as well as Jamaica and Cuba, New Orleans, Bermuda, suffered from a severe epidemic of yellow fever. In 1837, too, plague was frequent from Tripoli along the Barbary coast, to Alexandria, through the southern part of Syria and western part of Asia Minor, and continued up to the Danube. The mortality among the troops in the Ionian Islands, from fever, was four times higher than the previous year, and even at Malta itself remained pretty high.

year in Sicily. In 1836, the wave passed the isoclinal 70° N.; there was little heard of the epidemic in the North of Europe, but, in 1837, it seems to have prevailed at Dantzic, Berlin, and other places in the North of Germany.

In 1836, another cholerific wave passed the isoclinal 30° N. This year the disease was not very active in Northern India, but became considerably more so in the following one, though I have no definite information as to the districts it occupied. In 1836, in the western hemisphere, the epidemic broke out in Central America, causing great mortality in the St. Salvador district, on the Pacific side. It was also experienced at Honduras. In 1837, the wave entered the zone beyond the isoclinal 53° N. Cholera became epidemic in Malta, and reappeared in Sicily, Naples, Rome, and South of Italy, in a severe form, and it extended to Northern Italy and South of France; it was also experienced slightly in various parts of Egypt, in North of Syria, and Armenia. It does not appear that the wave caused any epidemic either in Europe or America to the northward of the countries here mentioned.

In 1837, plague had been prevalent from Tripoli to Egypt, and to the north as far as Constantinople. In 1838, this disease seems to have nearly disappeared from Egypt, but it continued severe at Constantinople, and in 1839, there seems to have been some at Broussa, on the east coast of the Sea of Marmora. The mortality from fever among the troops in the Ionian Islands and Malta rose considerably in 1839. In Great Britain, in 1840, fever was, on the whole, less frequent than the previous years, but was still prevalent in several places.

In 1838, a cholerific wave passed the isoclinal 30° N., and there was a considerable increase of mortality in Northern India. There was a slight epidemic in Egypt; whether there was any in Persia I have not heard. In 1839, the wave passed into the zone beyond the isoclinal 53° N., but I have no notice of cholera being epidemic anywhere in it, though the course of the wave was apparent in the Mediterranean by an increase of common or sporadic cases, and the following year in England and Canada.

In 1840, plague existed at Cairo, Alexandria, and through the Delta, as a pretty severe epidemic, also through Syria, extending into 1843. In 1841, plague was very severe at Erzeroum, and fevers prevailed from Bombay northwards, and even in the villages around Simla.

In 1840, the next cholerific wave passed the isoclinal 30° N.; there was a considerable increase of cholera among the troops in Bengal, but a great reduction among those in Bombay, and in 1841, the disease was more prevalent in Bengal, while in Bombay it was still further reduced.

Plague, as mentioned above, continued in Syria in 1842 into 1843. In the latter year fever was very prevalent from Agra northwards, and in Upper Scinde.

In 1842, another cholerific wave passed into the zone beyond the isoclinal 30° N., and there was a severe epidemic among the troops both in Bengal and Bombay. The following year its force was much reduced. I have no evidence of this wave having led to any outbreak of the epidemic form of the disease anywhere to the northward.

In 1844, the fever noticed the previous year seems to have continued in Upper Scinde at least unabated. The 78th Highlanders, between August and March, 1845, (stationed there) buried 669 of their num-

In 1844, the following cholerific wave passed the isoclinal 30° N. There was a considerable mortality from cholera in Northern India, but not much greater than the preceding year; but, in 1845, there was a very

ber, nearly all from this form of disease. At Ghazeepore, the 29th Regiment lost 85 men from fever in 1844; so that part of the year, at least, that disease must have prevailed much nearer Calcutta. In 1845, I have no notice of the prevalence of fever over Upper India, or in Persia or Egypt; but in all the Mediterranean stations the mortality from that form of disease was considerably less than in 1844, but in all of them it increased very much in 1846. In 1847, fever, which had increased perceptibly at the end of the previous year, became very prevalent in Great Britain and Ireland, and continued into the following year. Fever was epidemic in Prague and Upper Silesia in 1847, extending into 1848, and intermittents prevailed to an unprecedented degree in Sweden in the latter year.

I have no notice of the prevalence of fever in Egypt or Syria in 1846. On the West Coast of Africa there was the epidemic in the "Eclair," and at Bona Vista in 1845, which was followed up in 1846 by severe fever at Teneriffe, and a large increase in all our military stations in the Mediterranean. In 1847, as already stated, fever was epidemic in Prague and in Upper Silesia, in Great Britain and Ireland, in Sweden, and in all these this form of disease prevailed through the greater part of 1848. In 1847, fevers were also very prevalent in New Orleans and the Southern States of America, and in New large increase. This year the epidemic was severe in the north-west provinces of Bengal, the Punjaub, and it invaded the hill districts; it also affected Cabul, extended through Persia as far as the Tigris, and is said to have been at Bokhara in November. In 1846, the epidemic recommenced in the North of Persia, it affected several places in Armenia, passed beyond the Caucasus, along the west side of the Caspian, and traversed the country of the Kirghis Tarters between the Sea of Aral and Orenburg. In 1847, the epidemic was at Moscow in September. In 1845, this wave overspread the zone beyond the isoclinal 53° N., and in 1846, that beyond the isoclinal 70° N. The severe part of the outbreaks mentioned in this paragraph were, consequently, under the second year of the wave in each.

In the beginning of 1846, a cholerific wave passed the isoclinal 30° N. The mortality in the Bombay Presidency was very great, and the sudden outbreak at Kurrachee took place, and the epidemic extended through Persia, and as far north as Diarbekir, and to the westward as far as Aleppo; Aden and Mecca were also included in the epidemic area. In 1847, the wave passed beyond the isoclinal 53° N., and the disease overspread European Russia as far as Moscow. In 1848, the wave passed the isoclinal 70° N., and cholera became epidemic in Petersburg and Finland. About the beginning of Octo-

York, Buffalo, and Canada, and in all of these continued active through most of 1848.

ber it commenced at Bergen, in Norway, and in England and Scotland, and, about the middle of November, in Belfast, and became a severe epidemic in 1849 throughout Great Britain. The "New York," with emigrants from Havre to New York, was attacked at sea, near the American coast, on the 25th of November, and the "Swanton," also with emigrants from Havre to New Orleans, about the 29th of November, within 10 days of her arrival. The disease commenced in New Orleans apparently on the 13th of December, and by May, 1849, was at Chicago and New York and intermediate districts, and the following months in Canada.

The returns of disease among the troops in the Mediterranean and North American Colonies not having been published from 1847, to 1858 inclusive, the information as to fever through the Mediterranean and in Canada is imperfect, and the analysis could not be carried out with advantage, owing to the scantiness of the information.

In 1864, a cholerific wave passed into the zone beyond the isoclinal 30° N. This wave reached Bombay towards the end of August, 1863, and its first year thus extends from September 1863 to August 1864, inclusive. The mortality there from cholera in that period was 4,813, as against 2,358 for the preceding 12 months, the second year of the previous wave, showing distinctly a great aggravation of the causes of the disease under the advancing wave.

In 1864, east of the Ganges, fever was very prevalent in the gaols, and population gener-

East of the Ganges, cholera was not epidemic in 1864 farther north than Lucknow,

ally, from Lucknow, northwards; to the west of the Ganges, it was also very severe at Agra, and prevailed at Nusserabad, Neemuch, and as far as Mhow and Baroda, and included Umballa, Lahore and Peshawur, to the north. In 1865, the southern margin of the fever field to the east of the Ganges maintained the same position as in 1864, and cholera did not advance; to the west of this river, the southern margin of the fever field receded to between Agra and Delhi, but still embraced Ajmere and Pahlunpoor, while, in the Punjaub, it continued as prevalent as the previous year.

A febrific wave was passing to the northwards about this time, the manifestations of which it is necessary to describe. This wave passed the isoclinal 30° S. on the 1st of January, 1863, and we have indications of its influence in the severe and prolonged fevers Baker and his companions suffered from this year, about 3° N. of the Line, when exploring the sources of the Nile. In 1864, this wave passed into the zone north of the isoclinal 0°. There was this year a considerable increase of mortality from fever among the troops in Ceylon and in Burmah. At Aden fever was considerably more prevalent among the troops than in 1863, though it caused much the same mortality. There was a terrible outbreak of plague, apparently, at Khartoum, at the junction of the White and Blue Nile, while at Cape Coast, Fyzabad, and Goruckpore, while west of that river, Allahabad, Suagor, and Mhow, indicated its northern limits; but sporadic cases were reported at Bareilly, Umballa, Lahore, and Peshawur. In 1865, the epidemic made no advance to the east of the Ganges, but west of it its area extended so as to include Agra, Ajmeer, Neemuch, and Baroda, while a few sporadic cases occurred in the north of the Punjaub, as in 1864. The epidemic was felt slightly at Kurrachee, and passed along the south coast of Persia as far as Lingar, at the entrance of the Gulf, but nowhere penetrated the interior, as it had done in previous invasions. Early in 1865, the epidemic was raging at Mokullah, a port on the south coast of Arabia, 280 miles east of Aden; and it seems to have appeared at Hodeida, on the Arabian coast of the Red Sea, in lat. 15° N., about the same time. In the end of April or beginning of May, cholera was recognised as prevailing among the Pilgrims at Mecca. On the 21st of May it was on board ship at Suez, on the 2nd of June at Alexandria, and made its appearance at Malta on the 20th, at Smyrna on the 24th, and in the beginning of July it showed itself, almost contemporaneously, at Jaffa and Beyrout, to the east of the Mediterranean, at the Dardanelles and Ancona to the north, and at Valencia to the west. In 1865, this wave overspread the zone beyond the isoclinal 53° N., and the epi-

in Western Africa, the troops suffered very much from fever. In 1865, when this wave overspread the zone beyond 30° N., there was a severe epidemic of fever at Hong Kong; it continued, as mentioned above, in Upper India, and a malignant typhoid fever raged in the marshy districts on the Euphrates during the summer months. Fever seems also to have been very prevalent at Beyrout during summer, and at Broussa, on the south of the Sea of Marmora. There was also, in the early months of 1865, an uncommon prevalence of low fever, with typhoid symptoms, at Cairo. On the West Coast of Africa there was much fever at Sierra Leone this year; also in Demerara and Grenada, in the West Indies. At Malta and Gibraltar, too, in 1865, there was a decided increase in the mortality from fever, though it was still moderate. In 1866, when this wave passed into the zone beyond the isoclinal 53° N., there was a considerable increase of fever among the troops at Malta, though at Gibraltar there was much less. At the Gambia, this year, fever was prevalent and fatal, also among the troops in Demerara and Trinidad, and prevalent, but not so severe, among both troops and inhabitants of Barbadoes.

demic passed to the northward with considerable regularity; it was experienced at Constantinople at the beginning of July, in the Black Sea, at Trebizond to the south-east, and Kustendji on the west, and for some distance along the Danube in the first week of August, at Odessa on the 6th, and Kertch on the 17th of that month. It reached Berditchef, in the government of Kiew, in Southern Russia, on the 27th September, and Altenburg, near Leipzic, about the same time, or a little later. The disease also prevailed in Italy and South of France during summer, and reached Paris about the 15th of September; it also extended through Spain in the course of summer and autumn. In the West Indies, the Island of Guadaloupe was attacked on 22nd October.

It is worthy of observation that, though about 26,000 pilgrims landed at Suez between 20th May and 22nd June, most of whom passed on to Alexandria, and among whom several cases of Cholera occurred during transit, yet the first manifestation of the epidemic in Egypt was at the farthest point they reached. From Alexandria, the epidemic area extended to the eastward and southward, so that Aboukir, Tanta, and Cairo were included in it on 17th June, Zagazig and Mansoura, on 20th, and Damietta and Suez (the inhabitants) on 26th; along the Nile it embraced Minieh on 30th June, and Kenneh, lat. 26¼° on 23rd

July. At Toussoum, on the Suez Canal, upwards of 40 miles from the latter town, there was an isolated outbreak, commencing on 16th July, among labourers occupied in excavating earthwork—always a dangerous occupation during epidemics, and near Suez itself, labourers similarly employed were affected with choleraic diarrhœa in June, and one case proved fatal among these on the 22nd of that month. This movement of the epidemic eastward was not confined to Egypt, but was observed in Syria and Southern Russia as well. Though cholera was at Jaffa and Beyrout in the first week in July, it does not seem to have reached Nablous until considerably later, and though a few sporadic cases were said to have appeared in Jerusalem prior to 22nd August, it was about the 10th October before the epidemic fairly declared itself there. At Damascus the first case was in August, and at Aleppo the disease commenced on 11th August, while, farther to the east still, Bagdad had its earliest cases on 25th September, and Mosul on 23rd October. In Russia, though the epidemic had been at Odessa in the first week, and at Kertch on 17th August, it did not appear at Taganrog, at the head of the Sea of Azof, until 12th October, and on 14th November there was an outbreak at Zadonsk, to the north of Voronez, and nearly on the same meridian with Taganrog.

Towards the end of 1865, the

epidemic ceased to advance northwards, but remained in a subdued form in France and some other parts of Europe. In 1866, however, when the wave passed into the zone beyond the isoclinal 70° N., as soon as the weather began to get mild, cholera again became active, and the epidemic gradually crept up to and passed that line, in Russia, Sweden, and Norway, Great Britain and Ireland, and appeared in the United States, not only from New York and Boston to Chicago, but embraced Savannah, New Orleans, and Galveston, in the South. It was scarcely experienced in Canada, however, there having been 13 cases only in Stratford, a small town in the western part of the country, in the month of October.

In the West Indies, cholera became epidemic a second time in Guadaloupe early in 1866, and appeared also in Martinique.

The details given above, when carefully examined will leave little doubt that the progress of epidemic cholera has been most materially influenced by the existence of fever fields in its course. It may be useful, however, to recapitulate the chief points before concluding the paper.

In Northern India, it has been shown that cholera frequently did not occupy the whole country, but existed with severity in one part, while another was free from it. The evidence at my disposal did not permit me to define these portions accurately for any epidemic anterior to that of 1864-5; it is obvious, however, that in 1818-19, in 1836, in 1840, and in 1844, the prevalence of fever in the western part of that country had materially interfered with the extension of cholera there. In 1864-65, the details in the Sanitary Report for Bengal and other sources, admit of the limits being laid down with tolerable accuracy, and they bring out the remarkable fact that, for two years in succession, cholera presented itself as a severe epidemic, from the sea on the west, to the foot of the Himalayas, its northern

limit just embracing Baroda, Neemuch, Ajmere, Agra, and Lucknow, and, during these years, the country to the north of this line was suffering from a severe epidemic of fever. As illustrating that point farther, I may mention that the fever diminished greatly in 1866, and remained comparatively low during most of 1867, and it was in the end of 1866 only the cholera passed the line it had reached in 1865, and became developed into the extensive epidemic of 1867 in this district.

The extension of epidemic cholera westward in 1823 seems to have been checked by impinging on a fever field extending from Egypt through Asia Minor. The circumstances in 1826-27 were similar, and again in 1828-29, in the latter of which years the disease had reached Astrachan, but the apparent diminution of fever to the west of the Black Sea, and probably also over South of Russia in 1830, permitted it to extend into Europe that year. In 1830, when a fresh wave was passing up, Egypt being occupied by plague, cholera did not appear there; but in 1831, when the plague diminished, cholera took its place, and the same year affected Smyrna, Constantinople, and Hungary, and, in 1832, north of France and Great Britain, the fevers previously rife in these countries having then decreased. In 1833, under a fresh wave, Lisbon, Havannah, and Mexico were affected, and, in 1834, the epidemic embraced Spain, Morocco, Great Britain, Sweden, and the United States and Canada. In these years the South of France and whole of the Eastern part of both shores of the Mediterranean were free from cholera; but as far as the evidence serves, fevers were very prevalent over them. In the West India Islands also fever increased in 1833, and in Jamaica the ratio of deaths from it, though lower than the previous year, still remained considerable, so that the non-extension of cholera to the south here, as in 1865, was coincident with a frequency of fever in the districts which escaped.

In 1835, the South of France and Italy, from the Alps to Naples, which had hitherto escaped, became affected with epidemic cholera; in 1836, Sicily was included. In these years, especially the latter, there was much fever in Gibraltar, Malta, and through Greece, Turkey, and Egypt. In 1837, under a fresh wave, Malta became affected, and the above districts again suffered, but Greece and Turkey escaped, the latter, at least, still being extensively affected with plague. The limitation of the epidemic for three years to France, Italy, Sicily, and Malta, in the centre of the Mediterranean, while the surrounding countries, as far, so our evidence goes, were occupied by fever, is a very striking fact.

In 1847, cholera was again at Moscow, and in the course of 1848 it spread through North of Germany, and reached Eng-

land late in the year. There had been much fever in the Mediterranean in 1846, and in 1847 this embraced Great Britain on the one hand, and Silesia on the other; the fever declined in Silesia in the course of 1848, and towards the close of the year in England, before the cholera acquired much force; the same feature was observed in Sweden and the United States and Canada.

The relations of the two epidemics in India, in 1864 and '65, has already been noticed. The immunity of Persia, Arabia, and West Coast of the Red Sea from cholera, in 1864, seems to have been connected with the prevalence of fever in these countries that year, and in 1865, when the epidemic extended to them, it established itself slowly, and only when the fevers previously occupying the various districts diminished. In this way, it did not radiate from Suez, where the pilgrims were supposed to have carried it, but from Alexandria, and then extended eastward and southward in a regular manner, not along certain lines of communication, but over the whole face of the country, and the same peculiarity was observed through Syria, and as far as the Euphrates.

In submitting these remarks to the Profession, it appears advisable to remove an erroneous impression regarding the scope of my views as to pandemic influences which seems very generally entertained, that is, that I regard them as the sole causes which produce epidemics. It is difficult to understand how this misconception could have arisen, as in all my papers on the subject it was expressly stated that, in accounting for the local developments of disease we have to deal with, both causes connected with persons, and with localities as distinguished from persons, must be taken into consideration, as well as those of a more general nature. It is true, that if the operation of general causes be conceded, the influence hitherto attributed to those depending on persons or localities must be profoundly modified, but their recognition, and the true limits of their operation, are no less necessary for a philosophical explanation of the generation and spread of epidemics than before the pandemic influences were thought of.—*Mr. Lawson's Pamphlet.*

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## II.—THE ATROPHIES AND DEGENERATIONS OF OLD AGE.

By Dr. W. Moxon, Assistant Physician and Pathologist to Guy's Hospital.

Little interest would belong to the atrophies of old age, if old age came upon us uniformly in all our components; we then need not study each part; but we age in systems. Some men, especially gouty men, have kidneys that grow old before their

time; every person over three years old has some old age about him, and we cannot over-estimate its importance. In different people this atrophy affects different systems; in some the lung ages and wastes early; in others the aorta or cerebral arteries; in others the bones or cartilages. The same fact is familiar enough in different people, in the falling off of the hair, which we know occurs in people without any weakness in intellectual and muscular vigour. Many insurance offices ask after the teeth and after arcus senilis, as if the failing of one system showed all to be weak; but no insurance office, as far as I know, cares for a man being rather bald, though this is quite as significant as the other signs sought. Arcus senilis has no relation to other fatty degenerations, so as to prove more dangerous states within, although it has been thought to have such a value. We age in systems; the cornea in one, the arteries in another. It is interesting to notice where the arcus begins; this points to its cause. You see the arcus first under the upper lid in the part of the cornea so covered and prevented from its function of transmitting light; a patch of arcus is very common there, evidently from loss of its function. In the same way, when the bones are old and fatty, you will find the fixed ribs soft when the false ribs are hard—the latter still doing duty more actively as levers in breathing than those fastened to the sternum. We mentioned that, just as hypertrophy passed into tumour, so atrophy passes into degeneration. There is a very sharp distinction between atrophy and degeneration; the former means wasting without change of composition; the latter means change of composition; but I cannot say this very natural distinction has much value, for we rarely see elements waste very much without change of their composition; the distinction is best realized in the case of baldness (atrophy of hair) and greyness (degeneration of hair). The two forms of senile imperfection of the lung are also like this in their relation—the one being a passive wasting away of air cells, leaving a simply thin, delicate, atrophic lung; the other a coarse, thickish, bloated lung deformed with blebs, and in its minute texture having undergone rather transformation of its elastic tissue to an inelastic or less elastic substance, with fatty degeneration of its epithelium. The first is a bald lung, the second a grey lung; and each kind has its own peculiarities of history and results. Except in these examples, we scarcely ever see wasting without degeneration of the wasting tissue into fat grains, or albumen grains, or lime or pigment. Some authorities feel a satisfaction in ascribing this change of the cell elements into fat to a slower flow of the liquids in their course within the cells; but the immediate cause of it is not likely to be made out until we know better what constitutes life.

There is a very close relation between the abnormal change to fat which always supervenes on mere atrophy and the normal fatty change which occurs in the life processes of some tissues. Thus, in the formation of milk, cells grow, become fatty, and break into a fluid charged with fat. What is normal in these cells of the acini of the mamma may occur abnormally in the muscular and elastic fibres of the arteries in atheroma, and in many other places, even to the connective and other tissues of the milk glands, so that in old people the mamma itself turns to a sort of milk. This is so far unfortunate; but it is not only in the case of strictly normal fatty destruction, as in milk formation, that we have a desirable result. Take the case of pneumonia, which you know now is anatomically a charging of the air vesicles of the lung with vast crowds of nucleated cells which stuff the vesicles.

The normal process of recovery from pneumonia consists of a fatty change of these cells until they behave exactly like milk cells, and burst into a milky fluid that is easily absorbed. Thus we get rid of them. The fatty change of these cells is the principal cause of the grey colour of old hepatisation, and it is the promise of recovery, not at all to be confused with purulent infiltration of the lung, which is always primary and never results, as a further stage, from grey hepatisation, as is often assumed. Purulent infiltration of the lung only occurs in low and bad inflammations, and is purulent from the first; pus will come from the crepitant lung. The fatty change that is normal in milk and fortunate in the pneumonic cells is a serious disease when implicating the arterial coats, through exhaustion of their vitality in old age, and it is a serious and significant thing when occurring in the epithelium of kidney as a consequence of the exhaustion from long-lasting inflammation of Bright's disease, where its presence makes the whiteness of the large white kidney —not that the fat is the disease, but it is an index of the inflammatory exhaustion. For, as we have seen that atrophy may be the direct result of inflammation in muscle fibres and nerve fibres, so it may also be the indirect result of inflammation in tissues little prone to such atrophy; indeed, no tissue is exempt from this. Excessively-prolonged irritation will at last lead to the wasting away and degeneration of areolar tissue of skin. In the leprous subject which we inspected two years ago, the skin and subcutaneous tissue, which had long been the seat of the leprous inflammation, had its elements wasted beyond microscopic recognition, and lines of fat and pigment took the place of areolar fibre-bundles, the skin being so rotten in consequence, that it tore when pinched. Long-standing inflammation so produces the results of senility.

Atrophy, then, like hypertrophy, stands in this relation to

inflammation—that inflammatory irritations produce it directly; but, on the other hand, either occurs without inflammatory irritations, and those cases, where they so occur, lead into opposite kinds of pathology—atrophy into senescence in decay, hypertrophy into juvenescence in tumour.—*Med. Times and Gazette, Dec.* 10, 1870, *p.* 667.

## DISEASES OF THE NERVOUS SYSTEM.

### 12.—ON THE CLINICAL OBSERVATION OF DISEASES OF THE BRAIN AND NERVOUS SYSTEM.

By Dr. THOMAS LAYCOCK, Professor of the Practice of Medicine, of Clinical Medicine, and of Medical Psychology and Mental Diseases, in the University of Edinburgh.

The law of direction of physiological activity or of the *vis nervosa*, applied to the investigation of diseases of the brain and nervous system, is really an application to clinical research of the well-known laws of reflex action. But very few are aware that the direction of physiological activity indicates also the direction of structural degeneration. For this reason, and because of its great practical importance, I must call your special attention to this general fact. As the late Dr. Waller demonstrated it to be the law of degeneration of both the motor and sensory spinal nerves, and showed its value as a means of both anatomical and pathological research, I have named it the WALLERIAN LAW. I am, however, responsible for extending its application from the spinal cord and nerves to the brain and nerves of special sense. I may say now that, on the report of Claude Bernard, Dr. Waller was awarded the prize of 2000 francs, given by the Academy of Sciences, for experimental physiology in 1856. You will find the extended views of the question in my psychological text-book. The facts are simple. If the two roots of the second cervical pair of nerves of an animal be divided (and this can be done in the dog and cat without exposing the cord), and it is allowed to survive a few days, certain results will follow. The posterior root between the ganglion and the cord will be found to have undergone degeneration, and also its continuation upwards in the cord; whereas, the anterior root and that part of the posterior root still in connection with the ganglion will be unaffected. It is thus shown that the ganglion influences the nutrition of the sensory or afferent nerves, and the anterior columns that of the motor nerves; in other words, degeneration goes on in the direction

of physiological activity. Serres observed the same condition in a case of disease of the Gasserian ganglion; he traced degeneration along the decussating fibrils to the opposite hemisphere. So that disease of a sensory nerve which implicates the sensory or intervertebral ganglia will extend centripetally or adcentrically as far as its ultimate connections, and not unfrequently, therefore, reaches as high as the hemispheres, the organ of perception and thought. It is in this way we can understand how insanity results from apparently very remote and unlikely causes. If the degeneration begins exclusively in one set of sensory or motor nerve fibrils, it may be, and often is, limited to that set all the way up or down. We thus understand how it is that in disease one set of fibrils is picked out, as it were, from the rest.

Another point of importance was shown by Waller—namely, that the intervertebral ganglion influences the nutrition of the distal sensory fibrils; for when a mixed spinal nerve is divided on its distal side, both motor and sensory fibrils undergo degeneration. Hence the conclusion that excessive activity or exhausting use of the sensory nerve and ganglia will affect the nutrition of the sensory fibrils. This seems to be the order of causation in certain kinds of locomotor ataxy.

The problem to solve being, then, the order of symptoms, we have to inquire in any case where in the nervous system the functional or structural changes began, and then follow the line of physiological activity. This is already done with much success in cerebral hemiplegias; but the sensory and trophical neuroses have had little attention paid them in this way. There are two such lines in all neuroses—namely, the direct and the decussating—whether the line of activity be centripetal and adcentric (sensory, afferent) or centrifugal and excentric (motor, afferent). In either kind, single nerves and nerve-centres may be affected, or many nerves and centres. Affections of the sensory nerves in the latter case influence central parts by what has been termed radiation of sensation—the phrase meaning diffusion, anatomically, of the degeneration among the centres —so that several functions are disordered. I will give you illustrations of these views.

An injury to a sensory or afferent nerve may be followed by varying centric disorder and disease. In traumatic tetanus the the spinal trophic system of the flexor and extensor muscles of the lower jaw, limbs, and trunk is so affected that tonic convulsions follow upon a slight touch or even change of temperature; or, the injury may cause a neuralgia and no tetanus, but spasms and epileptic convulsions; or, there may be no neuralgia, only illusive sensations, as an aura, numbness, and yet centric encephalic disorders result; or, there may be no change percep-

tible by the patient, and yet there may be various vesanise, such as the so-called "masked" epilepsy, mania, melancholia. I give you as an illustration an instructive railway case. On March 24, 1866, a house-agent and tax-collector, aged 60, weighing 22 stones, was holding on a railway carriage, when the guard crushed the end of his finger by shutting the door, so that a portion was squeezed off. He suffered much from pain and loss of blood, and reached home faint and exhausted. The finger healed, but in the course of a month after the injury he had a slight tetanic symptom, and in a few days after that a sort of fit. He now complained, also, of numbness and strange sensations in his hand and arm, twitchings of the face, and a sense of weariness and loss of strength, so that, although previously in robust health, he was unable to undergo even slight exertion without a feeling of fatigue. He resumed his office-work for six months, but got slowly worse, becoming highly nervous and dreadfully depressed. He had next numbness of the body and faintness; then by degrees his powers of speech, of motion, and vision failed; and at last he died, September 13, 1867, nearly eighteen months after the accident. This is one of the many kinds of injuries to the nervous system railway accidents cause. What was the probable pathological anatomy? First, degeneration of the afferent fibrils of the injured finger; then of the intervertebral ganglion; then radiation thence, either upon other ganglia, including the Gasserian, or else upon the sensory structures of the cord; next, as a sequel degeneration, either of the vessels, or lymphatics, or connective tissue, or of all, so that the functions of both the sensory and motor sides of the cerebro-spinal centres were abolished. The progressive disease, the age, and the great weight of the sufferer lead to the inference that the degenerations were diathetic.

The element of time is a very important point in the diagnosis and prognosis of this class of cases; the progressive degeneration may extend over several years. In July, 1868, I saw a captain in the Royal Navy, who, fifteen years before, when a midshipman, fell about eight feet as he was descending Table Mountain, Cape of Good Hope. He received a scalp wound, which bled freely, and he thought he must have been made unconscious. The surgeon of his ship examined, but found no fracture, and dressed the wound, which healed well. I found an extensive scar on the scalp over the curve of the left parietal region, and the surface slightly depressed. This had led some to propose trephining. Twelve years subsequently to the injury, he married, and shortly after had habitual headaches, with mental depression, increasing until he became profoundly melancholic. Rest from active duty restored him to compara-

tive health of both body and mind, but his manner continued to be peculiar. He, however, resumed charge of a ship, and so got involved in anxious and harassing night-duties off the Irish coast, watching the Fenians. This exhausting work induced a series of neuroses of the encephalon, which were progressively intensified into structural disease, until (when I saw him) he was weak of mind, incapable of movement, passed urine and fæces involuntarily, and had great difficulty of articulation, as well as an incapacity to express his ideas by appropriate words, although he easily smiled and laughed. Early in November of the following year he had successive fits of convulsions, became unconscious, and so died, sixteen years after the injury to the scalp. Long as this period may appear, I knew a major, whose insanity was attributed, and I believe rightly, to a scalp wound (sabre-cut) received at the battle of Waterloo, thirty years previously.

The relation of these exciting and predisposing causes to time —and long time—is one of the most important points in the observation of diseases of the nervous system. It meets you everywhere. Thus, the tendency to leprosy (which is a trophesy) will lie dormant in Europeans who have been resident amongst a leprous population, and be manifested several years after their return to Europe, when exciting conditions arise. If the constitutional tendencies be diathetic, and not acquired, as in the syphilitic and leprous, the climacteric period strongly predisposes. Any shock to the nervous system, sometimes comparatively slight in character, often serves to waken the dormant tendency into activity. Marriage, which was thus operative in the case of the naval officer, is specially influential when entered upon in old age, or at the climacteric period, which practically are the periods when constitutional tendencies to degenerations shew themselves. Various diseases of organs which have direct sympathies with the brain and cord are thus apt to be exciting causes.

The decussating anatomy of the nervous system must be noted to observe well these various causes. In the optic nerves and their commisure we have an illustration of both the direct and decussating anatomy of all the sensory nerves of the trunk and limbs. But afferent nerves, which do not naturally subserve to sensation, will influence the opposite side of the central axis. Thus, there is a connection between wasting of one ovary, or one testis, and wasting of the opposite half of the cerebellum. I think I have seen cases of a like relation between disease of one kidney and the opposite half of the cerebellum. An affection of one knee, or one foot, will affect the kidney or the ovary on the opposite side. Nay, it would appear that the milk in the two mammæ is secreted differently from this uni-

lateral action of the nervous system. It is well known that infants will refuse the milk of one breast and take that of the other. The decussating influence of the injured ciliary nerves of one eye in inducing inflammation of the other is another instructive example of these trophical sympathies.

The law of degeneration, as a retrocession to a lower kind of tissue-change, may be applied to the pathological anatomy and chemistry of organs and tissues, and enable us the better to understand the neurotic causes of morbid changes. Thus, nervous debility, considered as a deficiency of trophic energy, will coincide with anatomical and chemical tissue-changes of a lower type. The production of uric acid, the *materies morbi* of gout, is an illustration: it is a normal chemical product of transformation of tissues in reptiles and birds; in man it is abnormal, being a retrocession from urea. So lactic acid appears to be the result of a retrocession in muscular transformation from a higher compound. Carbons and hydrocarbons as pigments and fats follow the same law as to place of production; amyloid degenerations are chitinous.

In diathetic anatomy, diagnosis, therapeutics, the evolutional law of tissue-anatomy upon which I found my clinical view of diatheses is an important guide to pathological inquiries. Nothing can be more vague than the current doctrines. Eminent French Physicians speak of a "herpetic" diathesis or of an asthmatic deathesis—phrases that have really no definite application to tissue-changes, and are, I think, worse than useless. I must remind you that the word diathesis means a special putting together of the fundamental elements of the body, and has no regard to particular organs or viscera, except in so far as a particular tissue predominates therein. The general law to which I refer is that with which you have been made acquainted already, and is the order of evolution of tissues in the embryo. This order indicates the general or common pathological relations of fundamental tissues. Firstly, the germinal membrane appears as the common basis of all; then follows its division into the "mucous" and "serous" layers. Out of the serous layer is evolved the the whole voluntary motor apparatus of bones, muscles, aponeuroses, ligaments, and serous tissues; so that, as they are all related to each other by common origin, they are related to each other nutritionally and diathetically, and have probably a common relation to a trophical system. The heart and vascular system have a like common evolutional origin out of the vascular layer of the embryo, which, however, is a conjoint product, and has the conjoint qualities of the mucous and serous layers. In diathetic anatomy the difference is shown by the distinction between gouty degenera-

tions affecting the structure of the heart and arteries and of the synovial membranes of joints, and the rheumatic which involve the fibrous structures and fibrous pericardium, and aortic and mitral valves.

The hereditary tendencies to diathetic diseases and degenerations as thus defined and fixed on an anatomical basis are more easily comprehended when we remember that tissue-changes in plants are hereditary, and that consequently it is the regulative principle, as vis nervosa, which, in animals endowed with a nervous system, must be operative on the sperm-and-germ-cells. Now, a regulative energy, manifested as the "nisus formativus," is the special property of these minute portions of matter, and consequently it must be by a concentration of that energy, as vis nervosa on the genetic glands, that the peculiar property is supplied. If, therefore, the innervation be defective in regard to these glands, the regulative or evolutional power will be defective. It is I thus explain how congenital degenerations of structure and defects of function and form take place, from deficient vis nervosa in the parents, for in these sperm-cells and germ-cells, as in other tissues, the law of degeneration is retrocession to a lower type. The nature and results of that defect in brain-nutrition upon which hereditary insanity depends is a striking illustration of this law of hereditary disease and defect; it is the lower or animal appetites and instincts which crop out in hereditary insanity.

Before we consider special tissue-changes as trophesies, let us clearly understand what we mean when we speak of loss of tone, of nervous debility, and of defective innervation. It is clear, from what I have said, the phrases may refer either to defective vital energy in general, or to defective regulative energy specially, in which case it would be to the sensory portion of the nervous system we should look. Now, when we are made conscious of this class of changes, pain, languor, unease, and other like feelings are experienced. Hence, it seems useful to inquire with reference to two kinds of vis nervosa, the one as being a molecular energy, necessary, like heat, to all healthy tissue-work, the other regulative of its production and application. This latter is therefore needed, not to the end that the tissue changes shall take place—for we have seen that they can and do go on independently of nerve—but that they shall take place in their proper or normal order. For example, if a defect in a nerve or nerve centre (nervous debility) is followed by the production of heat or of uric acid in the tissue it innervates, these changes occur because the tissue is set free from the regulative restraint exercised by the nerve or nerve-centre. Now, I think clinical facts enable us thus to distinguish two kinds of

trophic vis nervosa with corresponding anatomical seats, for we can differentiate a regulative from an executive kind just as we distinguish a sensory and a motor. Here, again, the law of evolution helps us to a clearer understanding. Just as the trophic vis nervosa is an evolution and differentiation of vital energy, so that by which we are conscious and act volitionally is an evolution and differentiation of the regulative element of trophic vis nervosa. It is that regulative principle which, as manifested in nutrition and development, was, and indeed still is, termed, the *anima psuche*, soul; and as manifested in mental life, as the *animus*, mind. This unity and continuity of vital phenomena is the great truth of Medicine, as it ought to be of philosophy. I do not know in the whole range of the practice of Medicine questions of greater importance than those comprised in the relations of nervous debility, in the scientific meaning of the term, to states of consciousness. It implies not only a true knowledge of the relations of pain to disorder and disease, whether in the merely corporeal forms, as tenderness on pressure, hyperæsthesia, neuralgia, and of diminished and abolished sensibility, as anæsthesia—for pain and anæsthesia mean practically changes in the vis nervosa—but of all mental suffering. Pain and suffering are so commonly associated with disorder that the word disease is its synonym, and the ancient word pathology means primarily the science of suffering. Hence it is that the psychology of pain and suffering is so important a divison of the practice of Medicine; for how can we understand otherwise the commonest experience—*e.g.*, the use of opium and other sedatives in inflammations and painful diseases?

What, then, does pain mean in general?

When all the nutrient and mechanical work of the body goes on according to the rule or *norma* of vital activity, the functions are said to be normal and regular, and the body healthy. Corresponding to this normal bodily condition is a mental condition, the feeling of health and of being strong and well, which, if contrasted with the mental state that corresponds to what is abnormal, is pleasurable. It has been named variously, but perhaps the best term is cœnæsthesis, or common sensibility; morbid states of it are to be classed with the *æsthesiœ*. The feeling of ill-health generally, or malaise, or whatever name be given to that which accompanies what is abnormal, if there be a feeling at all, must have its seat in a like portion of the nerve-centres as the feeling of health, and this, in accordance with the principle formerly laid down as to the seat of all conscious changes, must be in the brain. Both states of consciousness correspond in regard to the body to that unity in regard to mind which the metaphysicians name the "ego"; hence these

facts prove, in conjunction with many others, that there is a trophic corporeal centre, or series of centres, just as there is a mental centre or series of centres. This we shall be able to fix in a well-defined basilar region of the encephalon, which includes the medulla oblongata, cerebellum, and cerebral ganglia. Whatever may be said of local pain or uneasiness may be said of these general feelings. Hence we must remember as an important fact in clinical observation that all pain, whether it be local or general, and however named may be illusive as to both seat and cause. It is so with the feeling as to health and unhealth; for, just as in a neuralgia, a person may feel as if he had disease of an organ when it is healthy, and *vice versâ*; or when strong and well may feel ill and weak, and have delusions as to the nature and cause of his illusive illness and weakness. The term "well" means, as thus used, that there is no disease of organs, or tissues, or blood, as causes of the feeling of illness, but that there is central disorder or disease as a neurosis, termed hypochondriasis and hypochondriacal melancholia. When it is not purely sensorial the painful state is best named dysphoria. These centric suffering states (phrenalgiæ) are often as painful as neuralgiæ. I have known hypochondriacs with the feeling of muscular debility, yet strong in muscular development, as incapacitated for labour as if really weak and ill in their muscular system. On the other hand, a patient seriously diseased may feel quite well—may have, indeed, a pleasurable feeling of health. This I term euphoria; it is this, when manifested in cases of phthisis, which has been termed the *spes phthisica*. A wider euphoria is seen in certain cases of insanity with paresis, in which, from a particular kind of morbid-brain nutrition, the patient thinks himself endowed with strength beyond estimate. A third class of cases are those with no sense or feeling; they have apathy or aphoria. Such persons, when very seriously ill—ready, in fact, to die—make no sign of feeling ill. This state occurs in dangerous cases of epidemic cholera and in fevers of a bad kind: the patient will go about wholly unconscious of serious illness or of his impending death.

What is said of these general bodily feelings, as commonly altered in disease, applies equally to special bodily feelings and conditions. Exaltation, perversion, abolition of sensibility have their respective trophic conditions. The results of anæsthesia of the pulmonary system, with evolution of motor vis nervosa, are seen in another form than the *spes phthisica*, when tubercular meningitis comes on in the course of a phthisical case. Even with large vomicæ the cough and expectoration will cease, the voice, as the patient raves deliriously, becomes loud and strong, and the corporeal strength marvellously developed—

conditions all due to changes in that basilar trophic region I have hinted at. On the other hand, there are cases of insanity in which the central trophic region is defective in both sensory and executive or motor vis nervosa, and the results are wholly different, manifested as low forms of pneumonia, sometimes ending in gangrene of the lung; precisely for the same reason that sloughing occurs in certain kinds of palsy, as in typhus and paraplegia.

The clinical rule deducible from these considerations, is, that when we desire to ascertain the causes and consequences of trophic nervous debility, we inquire whether it is the regulative —*i.e.*, sensory vis nervosa that is deficient—or the executive—*i.e.*, motor. Excessive use (functional activity) often determines this. Pleasurable sensory excitement, if excessive, is a using-up of the sensory or regulative vis nervosa. This is one cause of the debility induced by sexual excesses, and of locomotor ataxy as a special consequence of that debility. Pain, too, when excessive, is exhausting. Excessive thought, without anxiety, uses up the materials subservient to sensory excitation; and in this way the regulative energy as regards organs and tissues may be defective, and neurotic disorders of all kinds follow. But excessive thought, with mental anxiety, care, and pain, as grief, is much more exhausting, and therefore more commonly followed by trophesies. In the exhaustion caused by sexual excesses of males a third element is added—viz., the waste of a highly evolved tissue analogous in chemical composition and in vital endowments to brain—viz., the sperm-cell proper. In the sexual excesses of the female, this cause is not so operative, but chiefly the sensorial exhaustion of excessive pleasurable excitement. In like manner, the loss of blood, as in hemorrhages, or of albuminous nutrition, as in albuminuria, or the want of proper food, enfeebles both the regulative and the executive portions of the trophic system.

Induced in these and other ways, nervous debilty causes a variety of both local and general diseases of organs and tissues, which necessarily differ almost infinitely, according to the kind of tissue and the portion of the nervous system involved, and the causes thereof. It will help greatly to understand and classify and treat these if we know what is general as to nerve and tissue, and what is purely local. For example, there is a whole class of gouty tissue diseases which are essentially neurotic, and to be separated from those in which there are local changes as causes. Before inquiring, however, into these, we must first consider tissues in their relations to the nervous system, and then mark out a clinical trophic anatomy as a guide to etiology, diagnosis, and therapeutics.—*Medical Times and Gazette, March* 18, 1871, *p.* 301.

## 13.—A CASE OF TETANUS SUCCESSFULLY TREATED BY HYDRATE OF CHLORAL.

By Dr. A. G. LAWRENCE, Chepstow.

[The patient was recovering from myelitis, when some movements of the spine produced acute pain up the spine into the back of the head.]

Early on the morning of the 30th I was summoned to my patient, and was told she had passed a very restless night, and had not slept till 6 A.M., and awoke at 6.30 with her jaws firmly fixed and unable to open her mouth in the slightest degree. She complained of great stiffness and pain in the neck and jaws, accompanied with sharp spasm. The muscles of the back and legs were also rigid, the spasms never entirely ceasing, but being aggravated every quarter of an hour, and then partially subsiding. There was no hardness or rigidity of the muscles of the abdomen. Her voice was husky: she had the greatest difficulty in swallowing, and the saliva trickled down her chin. She had a careworn expression, and anxious painful smile. The sensibility of the skin was so much diminished that she did not feel the effect of the blisters which had been placed on the jaws the night before, and which had risen considerably. I immediately ordered three drachms of Ferris & Co.'s syrup of chloral hydrate in half an ounce of water every three hours, and, should the spasms relax, as much beef-tea and milk to be given as she could take. Half an hour after the first draught a feeling of glow had come all over the body, and, from being exceedingly cold, she became quite warm; the tonic spasm of jaw and legs began to subside, and she was able to open her mouth sufficiently to admit the handle of a teaspoon, and could with difficulty swallow a little beef tea, though the act immediately excited spasm. After the second dose the pain and spasm in the legs and jaw were much lessened, and she was able to open her mouth sufficiently to admit the end of a pencil-case; but no sleep was produced, although a calming influence had set in. After the third dose the spasm and pain subsided, so as to allow the mouth to be opened wide enough to admit a teaspoon, and beef-tea and egg-flip were constantly given.

Dec. 31. I found my patient had slept all night; the spasm and pain in jaws and legs had entirely gone; she was able to swallow well without producing spasm, and only complained of great soreness of the muscles of mastication and of the lower extremities; she was, however, quite unable to use the former. Has had one dose of chloral this morning, which is to be repeated at bedtime.

My patient continued to improve till Jan. 3rd, four days after, when, in having her bed made, the maid wrenched her back,

which in one hour brought on all the former symptoms in an acute form. The chloral was again administered every three hours, and after the second dose the tonic tetanic symptoms once more entirely subsided.

The point of greatest interest in this case, next to the favourable influence of the chloral, is the production by shock directly affecting the spinal cord of the train of tetanic symptoms which are commonly due to traumatic peripheral irritation. This, no doubt, is to be accounted for by the antecedent myelitis. My patient has continued to improve under a tonic treatment, with chloral only at bedtime, which she describes as producing a most pleasant feeling, and in half an hour after taking it a warmth comes, first in the hands and arms, then in the feet, and in three-quarters of an hour it produces an uninterrupted sleep of two hours. She then awakes to change her position, and falls asleep again, arousing in the morning very much refreshed, free from headache, sickness, or other uncomfortable feeling, and says that she would like to live constantly under its influence. I have tried in her case every anodyne in the Pharmacopœia, all of which deranged the system so much that I abandoned their use; but the chloral has admirably answered my purpose in procuring for my patient a good night's rest without any drawbacks.—*Lancet, March* 4, 1871, *p.* 303.

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## 14.—CASE OF TETANUS NEONATORUM TREATED WITH CHLORAL HYDRATE.

By Dr. WIDERHOFER.

Dr. Widerhofer showed lately to his class a child of three months old, which was attacked by tetanus neonatorum at the end of the first week after birth, and was treated with chloral hydrate in doses of one and two grains at the time of each onset of convulsions. It was in danger for a fortnight. During the intermission of the spasms it was fed from the breast by its mother. It is now a fine, healthy-looking child. This is the sixth case (out of ten or twelve) that Dr. Widerhofer has had of recovery under treatment by chloral. Under all other methods all his previous cases died. Considering that Vogel, and other great German authorities on children's diseases, had quite recently never seen a case of this affection recover, such a success must be taken to indicate a real advance in therapeutics. Dr. Widerhofer gives from two to four-grain doses of chloral by the rectum, if the infant cannot take it by the mouth.—*Lancet, March* 18, 1871, *p.* 379.

## 15.—ON THE USE OF OPIUM, BROMIDE OF POTASSIUM, AND CANNABIS INDICA IN INSANITY,

ESPECIALLY IN REGARD TO THE EFFECTS OF THE TWO LATTER GIVEN TOGETHER.

By Dr. T. S. CLOUSTON, Medical Superintendant of the Cumberland and Westmoreland Asylum, Carlisle.

[Dr. Clouston states that he has given bromide of potassium alone, or in conjunction with Indian hemp, in fifty-one cases of various kinds of insanity. In all of these cases the drugs had a fair trial, for a sufficient length of time. Very good results were obtained in a considerable number of cases.]

If to a patient whom one has known to have had regular attacks of periodic mania for years, we give a medicine at the commencement of an attack, and the patient's excitement ceases, contrary to anything known in the history of the case before, then I think we may fairly conclude that the medicine and the absence of mania are cause and effect. If in a case of mild melancholia at the change of life in a woman, the disorder has existed for a year and a half, if most of the remedies ever before recommended for that class of cases had been tried and had failed to do good, and if at last the bromide of potassium procures sound sleep, and immediate visible improvement in appetite, weight, and mental state, surely some credit may be given to it. But if in this same woman its use is intermitted, and all the symptoms at once return, and again immediate improvement follows its employment, so that the patient becomes able to employ herself as she never did before since her illness, and through healthy employment gains in flesh and strength, and gets quite as well in three months as ever she was in her life, surely we cannot deny to therapeutics a cure in the best sense of the term. Or if a cure cannot be expected, as in a case of general paralysis, if a mixture of bromide of potassium and Indian hemp so subdues intense excitement, that when not taking this medicine the patient is noisy, violent, destructive, sleepless, and rapidly losing weight, and when taking it he is quiet, semi-rational, dresses and eats properly, and remains in this state for six weeks, till the disease in its natural course passes into its quiet stage, I think here we have a palliative of great value and importance. Or if an old lady gets irrational, restless, sleepless, and unmanageable by her relatives, and if apparently the last alternative to sending her to an asylum has been tried and failed, until half-drachm doses of bromide of potassium and tincture of Indian hemp is found to subdue and quiet this irritability and restlessness, so that she can be quite well kept at home, for the month or two during which this excitement lasts, and until the ordinary dotage of old age

to which this excitement was a prelude, comes on, surely the physician's power was augmented, and the patient was unquestionably the better for the remedy he employed.

In acute mania I seldom found the bromide given alone do any good, or, indeed, have any perceptible effect. I gave it in all doses up to 120 grains three times a day, and I continued its use in some cases for a few days. But when combined with tincture of cannabis Indica the effects of the mixture were in many cases very remarkable. Sometimes if the excitement was very intense I began with drachm doses of each three times a day, or, in some cases, every three hours for the first day. In the cases in which the effects were good, they usually appeared by the end of the first day of its use. The patients become less restless, the shouting and violence were abated, and at night they slept. The skin, too, which is so often dry in acutely excited patients became more moist, and they perspired freely. The pulse usually lost in force. Indeed, this is the only objection I have to this mixture, that the force of the heart's action is undoubtedly lessened in most cases by it. But I have never seen a single case of syncope, except in one woman who fainted two hours after a dose, but soon recovered. The lessened force of the heart was shown, too, by the paleness of the face and skin generally. After the medicine has calmed the excitement the patient remains confused in mind. The intelligence and coherence of ideas, of course, do not usually return for some time. It is often sufficient if one or two doses per diem are given after the first day or two, and I have stopped its use altogether at that time—the patient remaining free from acute excitement. The greatest advantage of this sedative over every other that I have tried in acute mania was, that these patients took their food as well or better during its use as without it. Every one who has acute mania to treat knows that there are three great risks. The patient's appetite may fail, the excitement may cause complete exhaustion or death, or it may last so long that the power of the brain to become the medium of normal mental manifestations seems to be lost or impaired, and dementia results. There can be no doubt that the patients being got to take a large amount of nourishing food and stimulants is of the very first importance in all cases of acute mania, and it is the great risk of taking away the patient's appetite that prevents opium or henbane being more extensively used. Especially is this risk great if we give large doses of opium. It seems to me that the bromide and Indian hemp combined approached more nearly by far than any other drug to our great desideratum in treating acute excitement of the brain, viz. a medicine that will so alter or modify the morbid functions of the brain, that the patient will cease to exhaust all

his bodily energy in muscular movement and constant wakefulness, and will at the same time allow the reparative effects of rest and food to act quickly in restoring the normal nutrition of the cerebrum. In some cases complete recovery of the mental powers took place very soon indeed after the excitement was subdued; in others, the confused and incoherent state remained for a long time.

[Dr. Clouston concludes with the following summary:—]

A mixture of one drachm of bromide of potassium with one drachm of the tincture of cannabis Indica is more powerful to allay excitement than any of the other drugs or stimulants tried. It is more uniform and certain in its effects, more lasting, interferes less with the appetite; and to produce the same effect the dose does not require to be increased after long-continued use.

Single doses of opium tended to raise the temperature and to lower the pulse; single doses of the mixture above-mentioned to lower the temperature and quicken and weaken the pulse, of bromide of potassium alone to raise the temperature and lower the pulse, of cannabis Indica alone to raise the temperature and quicken the pulse, of whisky to lower the temperature very much and slightly to quicken the pulse, and of beef tea to lower the temperature in the least degree and to lower and strengthen the pulse.

By giving bromide of potassium and cannabis Indica together, not only is the effect of either given separately immensely increased, but the combination has an essentially different action from either of them given alone.

Bromide of potassium alone can subdue the most violent maniacal excitement, but only when given in immense and dangerous quantities, and its effects are so cumulative while so given, that after they have once begun to appear they increase for days after the medicine has been stopped, almost paralysing the cerebrum and sympathetic.

To produce sleep in mild excitement, one drachm of the bromide of potassium is about equal to half a drachm of laudanum. To allay maniacal excitement, forty-five grains of the bromide and forty-five minims of the tincture cannabis are rather more than equivalent to a drachm of laudanum.

Seven cases of chronic mania were treated for twelve weeks with opium, in doses rising gradually from twenty-five minims of the tincture up to ninety minims three times a day, and the results noted. After getting no medicine for several months the same cases were treated with a mixture of bromide of potassium and cannabis Indica in gradually increasing doses, and the results noted and compared with those of the opium treatment.

Under the opium treatment the patients all lost in weight continuously; their morning temperature was lowered and also their evening temperature, but the latter (which was too high, and its being high was a bad sign) very slightly, and their pulse was decreased in frequency. The opium allayed the excitement in the larger doses, but it soon lost its effect.

Under the bromide of potassium and cannabis Indica treatment the patients only lost in weight very slightly for the first six weeks, and after that they gained, their weight being more at the end of eight months' treatment than it was to begin with. Their appetites were not interfered with. Their temperature fell, especially their evening temperature, and the pulse was slightly increased in frequency and weakened in force, while the excitement was subdued, and the medicine showed no signs of losing its effect, even after being thus used for eight months. The maximum of good effects and the minimum of the ill effects of a sedative drug were thus obtained by using the bromide of potassium and the cannabis Indica in combination.

The bromide of potassium alone may be continued for months in doses of half a drachm three times a day, and the patients gain in weight and remain healthy in body.

Cannabis Indica being a diuretic, and the bromide of potassium being carried off by the kidneys, it is probable that the former in that way helps to prevent the cumulative action of the latter when given alone.

When the two are given together, the first symptoms developed are those of the cannabis Indica, but these soon merge into a state of drowsy calmness of the nervous system which is in all respects the opposite of nervous irritability.

Fifty-one cases of various forms of insanity were treated by bromide of potassium alone or along with Indian hemp, and the results were that eighty per cent. of these were benefited more or less in some way, and twenty-five per cent. were most decidedly benefited.

The milder cases of puerperal and climacteric insanity were sometimes remarkably benefited by drachm doses of the bromide of potassium given at night.

In some of the cases of acute mania the excitement was subdued in a few days by the bromide combined with Indian hemp in doses of from half a drachm to a drachm of each given three times a day.

In some cases of periodic mania and general paralysis all the worst symptoms of maniacal excitement were allayed by giving a mixture of bromide of potassium and cannabis Indica in doses of from half a drachm to a drachm and a half of each three times a day. This was continued in one case for nine months with the best effect.

In three cases of periodic mania, attacks were cut short by a mixture of the two medicines, or by the bromide alone. In one of these complete recovery followed.

Fewer cases of simple melancholia were benefited by the bromide alone or along with Indian hemp than any other form of insanity. Some were made worse by them, but in one case of this disease where there was great excitement and hallucination of hearing and suspected organic disease of the brain, the combination gave immediate and complete relief of all the symptoms for four months.

One case of senile mania was successfully treated at home by a mixture of the bromide of potassium and tincture of cannabis Indica, when she was to have been sent to an asylum. It seems probable that some such cases, and also patients with short attacks of mania might be treated by the same medicines at home, when at present they have to be sent to lunatic asylums, on account of the want of such a safe and powerful sedative.—*Brit. and For. Med.-Chir. Review, Jan.*, 1870, *p.* 203.

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## 16.—CASES OF ABDOMINAL NEURALGIA.

Under the care of Dr. HANDFIELD JONES, at St. Mary's Hospital.

The following series of cases is interesting as illustrating the diagnosis and treatment of a disease that it is not always easy to distinguish from other affections in which pain is referred to the abdomen, especially peritonitis, lead colic, and hysteria. The points on which Dr. Jones chiefly insists appear to be—1. That the region of the abdomen, probably its peritoneal lining, is liable to suffer from neuralgia and hyperæsthesia, such as prevail in other situations more notoriously prone to this malady. 2. That such neuralgic affections may very closely simulate peritonitis. 3. That they may be attended with high temperature. 4. That they may possibly pass into peritonitis. 5. That, in their treatment, opium (preferably in the form of enema), tonics, and restoratives, prove to be successful remedies.

The first case is that of M. W., aged forty, a married woman, who was admitted on the 9th September. She stated that three days previously she had been seized with pain in the lower part of the abdomen and the hips, so violent as to draw her double; since the day before admission it had been gradually subsiding, but with occasional aggravations. Whilst the pain was at its worst the abdomen had been so exquisitely tender that she could scarcely draw her breath, and whilst sitting up she had felt sick. On admission she flinched violently when the left side of the abdomen was touched, and she felt pain on

coughing or drawing a deep breath; when she turned on to the right side she experienced a feeling of dragging in the left; the tongue was thickly coated with white fur; the pulse was 78, and not very weak; the temperature 99·3°; there was no blue line on the gums. The right side of the abdomen was resonant, the left dull, and these sounds were not altered when she moved on to her right side. She had no appetite, but was very thirsty; the bowels were habitually confined; the urine was very red, and passed with pain; there was also pain on passing her motions; the catamenia had not appeared for six weeks, but she had a profuse leucorrhœal discharge. There was no history of lead poisoning. She said that she lived over some stables, and had experienced a similar pain several times during the past three or four years; she felt very weak, and the least thing threw her into a "fainting perspiration." She had not been able to sleep during the previous night because dreadful sights appeared directly she closed her eyes; "blood and all that sort of thing, mice running about," &c. The bowels were first relieved by an aperient, and then an enema containing twenty drops of laudanum was administered, and followed by a great abatement of the abdominal tenderness. The treatment was then made to consist of one-twentieth of a grain of strychnine, one minim of nitric acid, and ten of chloric ether, in an ounce of water, four times a day; with simple diet, a pint of milk, and four ounces of sherry. On the second day her sleep had been disturbed in the same manner as before; the abdomen was quite soft and only slightly tender. On the third morning the sleep had been sound and uninterrupted; the bowels were relieved by an enema, and the diet was changed to chop, pudding, and porter, the medicine to twenty grains of the saccharated carbonate of iron, three times a day. On the seventh she was still improving, but, on getting up, the pain and feeling of faintness returned slightly. In a few days more she was discharged.

The second case was that of A. N., aged twenty-six, a married woman with three children. She was admitted on Sept. 10th, and gave the following account of herself:—She was in charge of a house which had just been painted, and smelt strongly of paint and varnish, when, three weeks before admission, she was attacked with severe headache, which, two days later, was replaced by an abdominal pain which she described as having been "frightful" in severity, and since its onset she had been unable to lie on either side, or to obtain sleep at night. She had been feverish and without appetite. No other inmate of the house had been out of health. She looked thin and anæmic, had a copious leucorrhœal discharge, and was suffering such intense pain that six leeches were immediately applied to the

abdomen, and followed by poultices. On the 13th Dr. Jones found her very weak, complaining of great pain on the left side of the abdomen, but lying with the legs extended; the left flank of the abdomen was dull on percussion; the other regions were resonant. The pulse was 102, not very weak; the temperature 102·5°; the tongue moist. There was no flush on the cheeks nor eruption on the abdomen. The bowels had not been open for two days; the appetite was good; the breasts did not secrete much milk. She was ordered an enema of castor oil, to be followed, after the action of the bowels, by a simple enema containing twenty drops of the tincture of opium; also a draught of four grains of carbonate of ammonia and one drachm of tincture of bark, in an ounce of decoction of bark, three times a day, with a generous spoon diet. On the fourth morning she awoke in great pain from a sleep which had lasted from early in the previous evening. The pulse was 105; the temperature 103·4°. The abdomen was fomented, and another opiate enema administered, and she was ordered twenty grains of the saccharated carbonate of iron thrice daily, in addition to the draught. On the seventh day the pulse was 88, the temperature 99·7°, and pain of a much less severe character recurred from time to time. The diet was changed to one of meat and porter. On the eleventh day the temperature was 100°, and, in consequence of copious night-sweats, the ammonia-and-bark draught was abandoned for one containing four grains of quinine. Subsequently a chloride-of-zinc injection was made use of to check the leucorrhœa. Twenty-eight days after admission she only complained of an occasional return of pain towards evening. No blue line was observed at any time on the gums.

The third case was also a woman, thirty-three years of age, and single. She stated that she had been indisposed for a fortnight, and had kept her bed for a week. The first symptom had been pain at the upper part of the abdomen, all round it, and in the back, great pain also followed on taking food, and was relieved by vomiting. At the time of admission the pain seemed to be violent; she was found to be bending herself down and moaning; the tongue was moist, and tolerably clean; the pulse 80; the temperature 97·5° F. The abdomen moved a little in respiration, but the muscles were at times very hard and tense; there was no eruption. She had no appetite, but was very thirsty. On inquiring into her history, it was elicited that seven years before she had lived in a house whilst it was being painted, and that she suffered at the time some nausea; also, that two years before admission she had suffered for a short time a somewhat similar kind of pain. The teeth were found to be so encrusted and soiled with tartar that it was

difficult to determine whether or not a blue line was present. She also complained that for a year she had had pain about the rectum, accompanied by a frequent desire to defecate; but she had never experienced any pain during the passage of her motions. The uterus was found to be reclined, the fundus lying near, if not upon, the rectum. She was first ordered a grain of opium every four hours, ice to swallow, poultices to the abdomen, and a subcutaneous injection of ten minims of solution of opium; but neither these remedies, nor an opiate enema which had been administered, were found on the following day to have afforded relief. She was found to be twisting and writhing in bed, although the abdomen bore pressure fairly well. The pulse was 72, and not weak; the urine clear and of high colour. She was then ordered a drachm of tincture of valerian and four grains of carbonate of ammonia, in an ounce of infusion of valerian, thrice daily; but she obtained no sleep during the following night, severe pain being excited every time she moved; and, in the morning, the abdominal walls were found to be hard and retracted. The temperature was 98·9° F. An opiate enema was followed by relief, and a grain of opium was ordered to be taken every two hours. On the eighth day two grains of iodide of potassium were added to the draught. On the fifteenth she was ordered, in addition, twenty grains of saccharated carbonate of iron, thrice daily. On the sixteenth day the pains became regularly paroxysmal, returning morning and evening at half-past ten o'clock. On the same date she began to sweat a great deal at night; ten grains of quinine daily, in two evening doses, reduced the severity but not the duration of the attacks. The amount of quinine was then doubled by giving four doses daily instead of two, and by the twentieth day she was free from pain and improving in general condition. After this the pains returned slightly, and a treatment as for lead colic was essayed, but, proving unsuccessful, was changed for citrate of iron and quinine, under which they soon began to disappear; and within little more than a month from the date of admission, though the patient suffered occasional returns of pain, the abdomen was soft and mostly quite free from tenderness on pressure, and she was discharged at her own request.

These cases, Dr. Jones said, were fair specimen cases of a disorder which is by no means rare, at least among the poorer classes. In calling it "abdominal neuralgia" he had followed unwittingly the example of the late Dr. Addison, who had written a very full and interesting paper on the same disorder in connection with uterine irritation. The term might be thought too vague, but he preferred it to any other, for the reason that it did not appear to him that any organ is specially affected, and that any part of the region in question might be

attacked. If he were pressed to localise the disorder more exactly, he should name the peritoneum as the most probable seat, partly on account of the quality of the pain, partly because this membrane is coextensive with its situation. Romberg had described a hyperæsthesia of the mesenteric plexus, but the description he gave of it seemed more applicable to colic than to the malady they were considering. A perusal of the cases, Dr. Jones thought, indicated that the resemblance of the symptoms, especially the pain, to those of peritonitis was quite close enough to make the diagnosis sometimes sufficiently difficult. Dr. Addison had said that the pain occasionally attacked the whole of the belly, exactly simulating acute peritonitis, and that he knew of no more puzzling disease. The points which would prove most serviceable as guides were the previous history, the temperature, the posture, the respiration, the pulse, and the physical signs. If the patient appeared to have suffered from any cause of exhaustion, such as lactation, profuse leucorrhœa, over-exertion, semi-starvation, or the like, a neurosis was a more probable result than inflammation. If the disease was of long duration, such as from two to three weeks or more, and the pain persisted with severity, especially without the development of any other symptom, it could not be peritonitis; the same would probably apply if there was a history of several similar previous attacks. If the temperature underwent no rise, it afforded strong evidence against the existence of inflammation, though a high temperature could not by any means be taken to prove the converse. The second case indicated a temperature befitting typhoid fever, and it was only the sequel which conclusively showed that the pain was purely neurotic. It had been supposed that neuralgia could not coexist with pyrexia, but he had recently seen a case of ephemeral fever in which the patient complained of agonising pain in the limbs, and the temperature rose to 103°; on the day following the pain had greatly abated, and the temperature was found to be normal. As regarded posture, the peritonitic patient was said to lie in a fixed position, instinctively avoiding the slightest movement; the neuralgic often turns and writhes about: the former abstains from using the diaphragm in respiration, the latter does not, or, at any rate, not to so great an extent. After a few days the physical signs would probably afford decisive information. The dulness in depending parts of the abdomen, due to effusion, is not present in neuralgia. So, also, abdominal distension may be looked for in peritonitis, but not usually in neuralgia; though Dr. Addison speaks of a very marked instance of the latter, in which the belly was as tense as a drum and exquisitely tender. The case had been regarded as one of chronic peritonitis, and the surface of the abdomen "presented

a most singular appearance from the thousands of leeches" which at various times had been applied. A very important question might be raised—viz., whether a primarily neuralgic disorder may not pass into an inflammatory. Dr. Jones's belief was that it might, and though he had not observed it in abdominal cases, yet there was considerable evidence, he thought, that such a change may occur in facial neuralgia. For further remarks on this topic he referred to his work on the subject. Some might be disposed to look upon these cases as examples of hysteria, and would have summarily dealt with them by the treatment which Sir T. Watson recommends—namely, purging, followed by an assafœtida enema. To this he altogether demurred. If any definite meaning was to be attached to the term hysteria, it must imply that the patient's will was defective, that she was not sincerely anxious to get well; that, in fact, she wanted moral treatment more than medicinal. Such, he was satisfied, was not the case with the patients in question. The pain felt was, as far as he could judge, as *bonâ-fide* a pain as ever racked a sufferer from sciatica or tic douleureux; they recovered speedily, and under such treatment as was known to benefit neuralgia elsewhere.

The occurrence of some amount of nocturnal delirium, and that of a terrifying and distressing kind, in the first case, Dr. Jones added, deserved remark. Such disorder was, if he might use the term, quite *homogeneous* to neuralgia, both affections having their root in a feeble paretic state of nerve-centres, and being, as it were, branches of the same stock. The derangement, in fact, was of the same kind in both, and the diversity of phenomena depended on the site of the morbid action; the intellectual centres being affected in the one case, the sensory in the other. There were also indications that the vaso-motor nerve-centres were involved, and suffered in a like way; for the patient in the first case complained of "faint perspirations"—i. e., perspirations attended with a sense of faintness; and in the second, of copious nocturnal sweating, precisely such as is met with in maladies of exhaustion.

Another noteworthy point observed, especially in the first case, and which Dr. Jones said he had not unfrequently met with in other unquestionable neuralgias, was the reproduction of the pain by exertion. This depended on the fact (for such he held it to be) that consumption of nerve-force in one centre, or, to speak more correctly, of material qualified to generate nerve-force under oxidation, diminishes its production in another, and so favours the recurrence of a disorder which is essentially dependent on the failure of nerve-force.

The special cause of the disorder in these cases could not be said to be clearly ascertained. It might be thought that the

second was one of lead-poisoning, as the patient had been exposed to the smell of paint; but he could not take that view. There was no notable constipation, and no blue line on the gums. The pain was not like that of colic; it was attended with fever, followed by copious night-sweats, and cured by tonics. He had never seen anything like this in the frequent cases of lead disease he had met with. The lactation and leucorrhœa no doubt had materially promoted the action of the exciting cause.

The diagnosis once made, the remedies to be used were plain. Opium to relieve suffering, tonics to restore strength, the action of both being *led*, rather than seconded, by repose and good nourishment. In doubtful cases, after ascertaining that the bowels had been sufficiently cleared, he advised the use of an opiate enema before applying leeches. The result of this means might be so satisfactory as to make the further treatment clear. On the other hand, it was to be observed that in a puzzling case Dr. Addison thought it an error on the right side (for an error it proved to be) to employ the remedies for peritonitis. The few leeches which were applied in the second case gave relief; but he, nevertheless, thought they were unnecessary and undesirable. It was by no means impossible for such means, by the temporary benefit they produce, to betray the practitioner into their repetition, with ultimately disastrous consequences.—*Lancet, Nov.* 19, 1870, *p.* 704.

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## 17.—ON THE TREATMENT OF CHOREA BY ETHER SPRAY TO THE SPINE.

By Dr. JOHN ROSE, Chesterfield.

[The plan of treatment pursued in the following case was suggested by two successful cases of chorea treated in the same way at Lyons, and reported in an early number of the Lancet for last year.—The patient was anæmic, and had had rheumatism, but there was no trace of heart disease.]

I used the anæsthetic ether spray specially prepared by Messrs. Burgess, Willows, and Willows, London. I applied it along the spine four or five minutes at each time; and after fifteen sittings a very marked improvement took place, followed by complete recovery, for which the girl's relatives were most grateful.

The case was a very severe one, and I have no doubt of the efficacy of the ether spray, which is, I think, worthy of further trial in this unpleasant and not uncommon disease, especially when treatment by the bromides of potassium and ammonium, preparations of iron, zinc, arsenic, conium, and, last but not least,

chloral hydrate fail. The late Dr. Alison, one of the most philosophical and best practical physicians who ever lived, used to recommend counter-irritation along the spine; and, after enumerating other modes of treatment, candidly said that it must always be remembered that in some cases, where several of these measures have been ineffectual, the disease has spontaneously subsided, particularly under the influence of country air and summer weather. The late Dr. Addison recommended electricity to be applied to the spine; and records several cases successfully treated in this way, after drugs had failed. "I believe," says our medical Cicero, Sir Thomas Watson, "that many cases of chorea—most cases—would at length get well without any aid from physic. I believe, also, that many of the boasted specifics have been quite innocent of any share in the recovery of the patients to whom they were administered; at the same time I am quite certain that treatment has a great influence over the disease." It has long been known that the shower-bath and cold sousing are of great value in chorea, often acting like a charm; and the *rationale* of the action of the ether spray, both as a sedative and a tonic, is, I presume, physiologically speaking, partly at least, the same. It doubtless produces a strong impression on the nervous system, interrupting the series of morbid actions going forward. Perhaps some medical Newton may, ere long, arise who will be able to throw more light on this obscure subject.—*Lancet, Dec.* 10, 1870, *p.* 813.

## DISEASES OF THE ORGANS OF CIRCULATION.

### 18.—ON THE CONDITIONS OF DISEASE DEMANDING THE ABSTRACTION OF BLOOD.

By Dr. BENJAMIN W. RICHARDSON, F.R.S.

[There is no doubt that in blood-letting our forefathers held a powerful means of cure, and we are in error in throwing over the results of their close observation and experience in this matter entirely.]

We may sum up the physiological effects of abstraction of blood under a few heads. There is, *firstly*, modification of the balance of the circulating and respirating systems; *secondly*, influx of fluids into the venous reservoir from the tributary supplies; *thirdly*, hydration of the colloid fibrine, and dilution of the saline parts of the blood, leading to tendency to separation of fibrine; *fourthly*, syncope; *fifthly*, convulsion of the muscles from removal of nervous control, with retention of the muscu-

lar irritability; and, *sixthly*, decline of animal temperature. From these facts we may turn to the application of bloodletting in cases of disease, and may ask, and try to answer the question, What are the conditions of disease demanding the abstraction of blood?

*The Practice.*—In considering the practice, the first thought is that those effects of bloodletting, which might be expected to be useful, are such as may be called mechanical effects: in plain, whenever the venous reservoir is under distention, so that due motion of blood and of function of part depending on motion of blood for function is disturbed, abstraction of blood is the most direct remedy. We will consider these cases.

*Cases of Lightning-stroke.*—The experience of many observers on the efficacy of venesection in cases of lightning stroke is entirely in support of the value of this remedial measure. In my lectures on lightning-stroke, I have recorded a case in which a man who had been rendered insensible by lightning was bled in the freest manner, the consciousness returning during the bleeding, and the recovery being secured. The older men in Physic, who always opened a vein in such cases, were apparently clear as to the importance and success of the practice; and within the last year I have made some experiences which in the most singular manner confirm this practice. From an observation made by Benjamin Franklin—that the most humane and rapid mode of slaughtering animals for food would be by a powerful electrical discharge—I was led to try if this really were a practical suggestion. I therefore had sheep taken to the Polytechnic Institution, and had them killed by a discharge from the great battery there. I found that full-grown sheep could be fatally and instantaneously struck down by the discharge from a surface of a Leyden battery of ninety-six feet. But here came the curious fact: when animals struck in this manner were laid on the bench after the shock, apparently dead, and actually dead if let alone, they would show the most convincing evidence of recovery if they were instantly bled freely from the neck. After the blood had flowed for a little time, the animals began again to breathe; they struggled as they do when they have not been struck, and they became so distinctly conscious that I had to give up the notion of recommending any change in the present mode of slaughter. In another case, where a smaller animal was struck down and rendered apparently lifeless, the opening of the external jugular vein was followed by a gush of blood, by immediate return of the respiration, and rapid recovery.

The *rationale* of the treatment in these cases is most simple. The effect of the lightning, or of the electrical discharge, is to

create expansion of the contents of the venous reservoir; there is liberation of gases from the blood; distension of veins in all parts; pressure on the brain in the closed cavity of the skull, and insensibility as the result of the pressure; distension of the right side of the heart, so that the muscular structure is paralysed; and temporary cessation of all motion and function. The opening of a vein at once relieves all this embarrassment; the heart is set at liberty to recommence its contractions; the nervous centres governing the process of respiration are relieved; and, if the derangement of the animal mechanism does not extend to actual disorganisation of blood, or to severe rupture of organic structure, when no remedy would avail, the natural functions are restored, and life is saved.

I take it, then, that in cases of insensibility from lightning shock, or electrical shock, the abstraction of blood by venesection is demanded.

*Sun-stroke.*—In sun-stroke the conditions of disease are closely analogous, if not identical, with those induced by lightning. One of my earliest experiences—an experience that has made me hold to the thoughtful practice of bloodletting—had reference to this disorder and its treatment. A man was carried from the harvest-field to the residence of a Surgeon, a relative of mine, insensible from sun-stroke. The patient was carried in like a dead man—unconscious, powerless. He was livid, but breathing at intervals, and there was still some audible motion of the heart. The Surgeon, one of the school of Clive and Astley Cowper, and one who had no doubt as to what was the right thing to do acted promptly. "He will recover if we can only get blood," was the remark to me; so the man was held up by his mates in a garden chair, a fillet was put round each arm, and a vein was opened beneath each fillet. At first the flow of blood was slow, though the veins everywhere were distended to the utmost, then the stream became more determinate, and at last copious, and the result was that in five minutes the man was breathing freely, was becoming conscious, was recovering. He was simply cured straightway, was able to assist himself to walk away, and without any other touch of Medical treatment was restored to full health in a few days.

I urge, then, that in cases of severe sun-stroke, when the venous reservoir is under extreme distension, when the large tense veins offer themselves, I had almost said, to the operator, there is a condition of disease demanding abstraction of blood. In America this fact seems now to be recognised, and the practice of bleeding founded upon it, in so far as it has been carried out, has been most successful. In England, at this

time, the practice is lost, and case upon case, during the heat of summer, is registered in the returns of mortality as death by sun-stroke, in which virtually nothing has been done at all to promote recovery—nothing, I mean, that is likely to be successful. Mustard, perchance, is applied to the limbs, cold to the head, a blister to the nape of the neck; a purgative, if the patient can be made to swallow it, is put into the stomach, or an injection is administered by the rectum. Of what avail these slow, these almost meaningless measures? By the side of the grand old remedy they are trifles, having neither theory nor practice to sustain them.

*Cases of mechanical shock.*—The teachings of the writers of twenty-five years ago, when the reaction against bloodletting set in, told greatly and effectively against the abstraction of blood after mechanical shock. It was teaching diametrically opposed to the learning and observations of centuries, but it had its way, and it has been generally adopted. It is, perhaps, not altogether bad teaching, but it is certainly too sweeping. Mechanical concussion differs from lightning-stroke and from sun-stroke in that it does not increase the tension of the venous blood. It paralyses the muscular system through the nervous system, and it is probable that after such paralysis it were best to endeavour to restore by the application of warmth and the administration of a stimulant, rather than by abstraction of blood. I would not, therefore, put forward bloodletting in such instances, although, at the same time, I cannot conceal the suspicion that, in some cases, the old men were right in trying to draw blood. It is certain enough, from the history of Surgery, that the fathers of our art, coming to persons who had been apparently so stunned as to be, to common observation, dead, were successful in restoring motion of blood through the vessels of the body by drawing a current of blood from an open vein. I do not press the point, however, because we have in these days no evidence to guide us, except the *litera scripta* of the past. But there is a condition of reaction after recovery from the paralysis of shock which may call urgently for abstraction of blood. In the condition of which I speak, the heart, recovering first from the general shock, pours into the feebly-resisting capillary vessels a full volume of blood, leading to what is called congestion of vascular organs, and to secondary mischiefs resulting from congestion. In these circumstances the pulse becomes resistant, the veins full, the breathing laboured, the body hot; while the unconsciousness from the paralysis passes into unconsciousness from coma. Twice, in cases such as I describe, I have seen the abstraction of blood from a vein, until the tension of the vessels was relieved, lead to the most perfect of results; and both theory and prac-

tice bring me to the conclusion that this condition is one demanding abstraction.

This same rule would apply to cases of shock following some large Surgical operations, and this view is confirmed by Mr. Spencer Wells, who, after the great operation of ovariotomy, has several times taken blood to meet particular and serious symptoms. I asked Mr. Wells to be good enough to give me a brief note on the general results of taking blood in his cases, and thus he writes :—

"As to bleeding after ovariotomy, my conclusions are that it is most useful when there is *mechanical* cause for suffering, as congested lungs and oppressed heart, or sudden loss of balance between heart and blood, owing to removal of a large tumour which contained large vessels and received a free supply of blood. But when we have true Surgical fever to deal with, which I suppose is either septicæmia or pyæmia, or one of the two opposed conditions of excess or deficiency of fibrine—and I think we are making out the different forms of fever with which these different blood changes are associated—then I have always found bleeding do more harm than good ; more than once it has seemed to me to be certainly the direct cause of rapid sinking."

I shall deal with the last point, noticed by Mr. Wells, further on.

*Cases of Reaction from Exposure to Cold.*—In cases where the body has been exposed to what may be correctly called the shock of extreme cold, we have, during reaction, a series of symptoms similar to those which follow the reaction after shock from a blow or concussion. We have the same hard pulse, tense vein, congestion. When these cases end rapidly, we usually return them as cases of acute congestion of the lungs. Well, in truth, they are attended, not with congestion, of the lungs alone, but of all the vascular organs; and when they do not terminate at once in death, but run what is called their natural course, they pass into conditions of disease—"inflammation"—of some particular organ: lung, kidney, liver, spleen. The ancients were accustomed to bleed in all stages of this condition—in which practice they were, I think, wrong. But there is one stage in which, when they bled, they were essentially right—I mean the stage of acute reaction, before there is any organic change of structure from congestion and from effusion through the distended vessels. To draw blood freely in this stage is simply to cure off-hand. I was taught this lesson of treatment from the experience of a very excellent Practitioner, twenty-five years ago. I have always followed it, when permission was allowed me; I have never seen a bad result from it; I have cured at once by it; and I have seen the worst results from the

omission of the practice. It is pitiable to hear, as is constantly heard, of young, strong active people, who, by accident or Surgical operation, might lose blood without any expectation of danger, to hear of such being allowed to die from acute congestion because it is not now the fashion to take the blood (which they might lose by operation or accident without anxiety) for the direct purpose of saving their lives. These are cases, unhesitatingly, in which the conditions of disease demand the abstraction of blood.

*Cases of Cardiac Congestion.*—There is a class of case in which the right side of the heart is dilated and enfeebled, and in which, under some special circumstance, as exposure to cold, excess of food or drink, barometrical change, there is sudden embarrassment of the circulation, oppression of breathing, tenseness of veins, and threatened death. In these cases the veins themselves stand out so fully that you could draw a fair amount of blood without the fillet, if that were desirable; the body is dusky, the eyes injected, the mind restless, the passive oppression intolerable. It happens often in these cases that the patients are advanced in life, or, at all events, are past the meridian, and on this ground a special objection is taken towards "reducing" them by abstraction of a few ounces of blood; so they are treated with stimulants, with sedatives, with chloric ether, with a blister, and in time they may get relief. They get relief by this process—that, out of the obstructed venous reservoir, a certain quantity of fluid passes into the cellular tissue of different organs by exudation, and a certain amount of further organic disease is thus committed. In these cases it is my practice, whenever I can get the permission, at once to relieve the tension of the venous reservoir by the abstraction of a few ounces of blood from a vein. The effects have to be seen to be fully accredited. There is no exhaustion from this process, but a sense of relief from overwhelming oppression; the heart ceases to struggle against the load it could not before lift up; the dusky colour of the body clears away; and the symptoms altogether are relieved, we may say practically, at no more actual cost than would follow if the venous reservoir were allowed to empty itself by passive transudation of serum, and with no secondary train of evils derived from exuded fluid in vital organs. I knew a lady who was bled under the condition I have named after her 80th year, and who was always relieved by the process immediately. I classify passive cardiac congestion from enfeebled right heart as a condition of disease demanding abstraction of blood.

*Abstraction of Blood in Cases of Apoplectic Coma.*—Our predecessors were led in all cases of apoplexy to abstract blood.

They made on this point a hard-and-fast rule, and the people had such confidence in the rule that they insisted on having it carried out. A hundred years ago a Surgeon or Physician who refused to bleed in apoplexy would have lost caste on all sides, and would probably have been considered guilty of malpraxis. Admitting that our predecessors committed some errors by this hard-and-fast line of practice, they were less guilty of error, I imagine, than we are in this day; for whereas they treated apoplexy and often cured it, we, as a rule, do not in reality treat it at all, but let it take its own course, resorting to a sinapism, a dose of croton oil, or an injection, for the sake of doing something in the right direction, but leaving the case to that most perverse of curers, *Nature*, which, in nine apoplexies out of ten, means that we leave the patient to die.

But suppose there be sudden coma, with or without convulsion, from sudden pressure on the brain, what are the means by which we can hope to relieve? If the pressure be due to exuded serous fluid in the closed cavity of the skull, our object should be to get that fluid back again into the general circulation. If the pressure be due to paralysis of vessels of the brain or to obstruction to the return of blood from the brain, our object should be to reduce the pressure, and to limit the volume of blood. If the pressure be even from actual effusion of blood, our object should be to expedite the formation of clot, the separation of serum from the clot. and the absorption of the separated serum. Does anyone think sinapisms, croton oil, or injections into the rectum are reasonable measures for these important purposes; or does anyone see anything very promising in nature of a corrective kind? Under mere nature, in such cases, as far as I know, the unsconsciousness remains, the body becomes very hot, the action of the heart increases in force, the skin and the mucous surfaces exude copiously, the bronchial tubes become loaded with water, the respiration gets impeded from the accumulation of fluid, the body sinks in temperature, with recurring convulsions; and death, in continued unconsciousness of the patient, closes the chapter. Turn round to bloodletting now as the remedy, and see what it offers. Say we do not always cure by it: Well! is it not better to cure sometimes than never to cure? And what, I repeat, does bleeding offer in apoplexy? It reduces the force of the heart; it reduces the volume of blood; it leads to absorption of serous fluid, if serum be exuded; and it leads to the formation of clot, if blood be exuded. Why, so rational, so purely scientific is the process, that if it had never been discovered, and some one had now discovered it, he—the discoverer—would have been looked on as one of the greatest men of this age of Physic!—*Medical Times and Gazette, Dec.* 17, 1870, *p.* 694.

## 19.—ON THE MURMURS ATTENDANT UPON MITRAL CONTRACTION.

By Dr. C. HILTON FAGGE, Assistant Physician to Guy's Hospital.

[In the year 1861, Dr. W. T. Gairdner published an account of the presystolic (or, as he prefers to call it, the *auricular-systolic*) murmur. Since that paper, the subject has attracted a good deal of attention at Guy's. Before describing the cases which have come under his observation, Dr. Fagge gives a brief resumé of the main characters which attach to such a murmur, which he speaks of simply as a direct mitral, avoiding the use of the word presystolic until he has more clearly defined its characters.]

1. The first and most important, although not an essential quality of a direct mitral murmur is its *place in the cardiac rhythm*. It is "*pre*systolic." The "first sound of the heart" is no longer the first audible sign of the heart's waking up from its quiescence during the pause; it is *preceded* by the morbid sound or bruit.

2. The direct mitral murmur has a special *seat*. It is loudest over the apex of the heart, and is generally confined to the region of the apex. It is inaudible or scarcely audible at the base, and is very rarely carried round the axilla to the back. If the stethoscope be placed over the third left costal cartilage the natural cardiac sounds are perceived (unless there be coexisting aortic disease).

3. The *quality* of a direct mitral murmur is, in most cases, peculiar. Almost all those who have written on this subject have remarked that the "presystolic bruit" has a *rough, churning* (or, as Dr. Salter calls it, "*grinding*") character, which of itself enables the auscultator to suspect its nature and origin.

Indeed, I do not find among my notes the record of any single instance in which such a murmur has been a soft souffle, or musical, as is often the case with systolic mitral and other murmurs, heard at the apex of the heart. A presystolic bruit is also very frequently accompanied by a marked palpable thrill or "frémissement cataire."

This rule as to the peculiar quality of the direct mitral murmur is, however, sometimes deviated from in a way which has not, so far as I know, been noticed by writers on the subject, but which is deserving of the utmost attention on the part of the practical auscultator. The murmur is occasionally so short and sudden that it resembles rather a tone, and is in itself hardly to be distinguished from the natural first sound of the heart.

In reference to this matter let me quote some points from one

case. A woman was admitted under Mr. Bryant's care with gangrene of the left leg and foot; it became a matter of great importance to determine whether there was any cardiac disease which would give probability to a diagnosis of embolism. On examination I found that the heart-sounds at the apex seemed perfectly natural. Just as I was leaving the patient I noticed that at one spot the apparent first sound was slightly prolonged; this led me instinctively to place my finger on the carotid artery. I then discovered to my surprise that this apparent first sound in reality preceded the carotid pulse by a distinct interval, and that the apparent second sound was really the first sound. The apparent first sound was thus shown to be a phenomenon of disease, a *presystolic* or *direct mitral* murmur. Upon this stethoscopic observation alone was based a diagnosis of contraction of the mitral orifice: a diagnosis subsequently verified by an autopsy. Dr. Moxon also examined the patient more than once during life, and agreed with me as to the nature and origin of the physical signs.

It is of course implied in the preceding paragraphs that we have a method of determining the exact time of the ventricular systole, and for this purpose I believe that there is no plan so satisfactory as that recommended by Dr. Gairdner, namely, feeling the carotid pulse at the same time that one listens to the heart-sounds.

The simultaneous appreciation of a pulse by the finger and of a sound by the ear is no doubt a matter of difficulty at first, and requires some special education of the senses. The most favorable subject for enabling one to acquire this little art in the first instance is a person, free from cardiac disease, in whom the heart beats slowly, at the rate of fifty or less pulsations in the minute. A few opportunities of practising on such an individual will, I believe, warrant the student in speaking with certainty as to the rhythm of any cardiac murmur, provided the rapidity of the heart's action do not exceed a certain limit.

An even smaller amount of practice will enable the observer to satisfy himself that the radial pulse follows the carotid pulse (and therefore the ventricular systole) by so great an interval that anyone who trusts to the pulse at the wrist for the determination of the exact time of the heart's beat must necessarily fall into frequent errors. The length of this interval varies under different conditions; it is especially marked in cases of aortic regurgitation.

Hitherto, however, many physicians have been in the habit of using the radial pulse to fix the time of the heart's beat. Hence it has happened that two very different mistakes have been made. On the one hand a diastolic murmur has been regarded as systolic, and when such a murmur has been heard at

the apex a case of aortic regurgitation has been mistaken for one of mitral insufficiency. Of this I have seen more than one instance. On the other hand, a systolic murmur has from the same cause been supposed to be *pre*-systolic; and this I believe to have been the reason why students have ascribed such a rhythm to many murmurs which I could not regard as otherwise than systolic. The error is one which needs to be carefully avoided, for nothing is more likely to cause doubt as to the possibility of diagnosing contraction of the mitral orifice than finding a wide mitral in a case in which a *spurious* presystolic murmur had been heard.

It has been asserted on theoretical grounds that the rigid conical tube into which the mitral valve is converted in so many cases of contraction must necessarily be unable to close, and hence that the direct mitral murmur must almost invariably be followed by a second, regurgitant murmur. The results of clinical observation, however, are absolutely opposed to such an assertion. In a very large proportion of cases in which a presystolic murmur is heard, the first sound of the heart is peculiarly clear, sharp, and ringing. It thus comes to resemble very closely the natural second sound, and this resemblance is of course strengthened by the fact that it is preceded by a prolonged droning murmur, which may easily be taken to be systolic in rhythm if the auscultator be not careful at the same time to feel the carotid pulse. I have long been familiar with the fact that I could myself easily thus mistake the "first" for the "second" sound of the heart; but I have only lately discovered that the same mistake is uniformly made by the students who have gone round the wards with me, and whom I have tested in reference to this point. When I have asked a medical ward-clerk, or even a house-physician, what he has heard on listening to a case in which a direct mitral murmur has been audible, and, even when he has known that I considered this murmur to be "*pre*systolic," his answer has constantly been, "I hear a rough murmur, and after that a sharp, clear *second* sound." But I have afterwards found no difficulty in teaching him to determine for himself, by placing his finger on the carotid artery, that this apparent second sound has in reality been the first sound.

I must say further that even when I had, by means of the carotid pulse, ascertained the systolic character of the sharp, clear sound heard in such a case, I have myself more than once found it impossible to determine the same fact by means of auscultation alone.

It is probable that the statements in the preceding paragraphs will surprise some readers, who have been in the habit of believing that a presystolic bruit always "runs up to" and is

continuous with a first sound, or a systolic bruit; and that the danger of overlooking the direct mitral murmur in practice arises simply from this continuity.

It appears to me, in fact, that in many of the current descriptions of this murmur too much stress is laid on the point in question. I will not, indeed, say that an actual interval exists between the presystolic murmur and the first sound, considering how small a space of time can be ascribed to such an interval. But this opinion I will venture to express: that a presystolic murmur is very often quite as distinct from the following first sound as is a systolic aortic murmur from the following second sound; yet no one has supposed that such a systolic murmur is likely to be confounded with a diastolic murmur.

When the first sound or the systolic element of a murmur has been determined, and it has been ascertained that another murmur or part of a murmur exists in the interval between the ventricular beats, the next point is to decide whether this murmur accompanies or replaces the second sound, or whether it is a new sound preceding the first sound—in technical language, whether it is a "*diastolic*" or a "*presystolic*" bruit.

Generally speaking, this point is very easily decided. The *pause*, during the period when the heart is at rest, separates all the sounds that accompany one beat of the organ from all those sounds which belong respectively to the beats before and after. And thus one has only to find out whether the "bruit" is separated by the pause from the following first sound or from the previous second sound. In the former case it is a "*diastolic*," in the latter case it is a "*presystolic*" bruit.

But not unfrequently the bruit is prolonged, and occupies nearly the whole pause. It may then be separated by no longer an interval from the second (or even the first) sound before it than from the first sound after it. Now if such a bruit be localised at the apex, if the second sound at the base be clear, and if the bruit be of the peculiar "churning" or "grinding" quality, it may safely be asserted to be a "*direct mitral* bruit," and to be caused by an hypertrophied auricle forcing blood through a narrowed mitral orifice. The name of "*auricular systolic*" may also be justly applied to such a bruit, but it cannot strictly be called *presystolic*. It ought rather to be called *diastolic*, quoad its rhythm; and thus Dr. Gairdner speaks of the murmur of mitral obstruction as sometimes "*ventricular-diastolic*," as well as "*auricular-systolic*." Further on, I shall have something to say as to the way in which such a prolonged direct mitral murmur is produced. But I wish here particularly to insist on the fact that I have found great bewilderment to arise in the minds of students from the careless application of the name *presystolic* to such murmurs, occupying nearly the whole pause. There can

be no doubt, I think, that this name will ultimately supplant all others as the designation for the murmur caused by contraction of the mitral orifice. But when it is used for all such murmurs without exception one ought to clearly understand and to explain that its strict meaning is departed from, and that the question of rhythm is then really overlooked and supplanted by other and secondary considerations.

To two of the considerations which come into play under such circumstances I have already referred:—the localisation of the bruit at the apex, and its peculiar churning or grinding quality. But there is yet another character which belongs to a large number of cases of contraction of the mitral orifice; this is the extreme variability of the rhythm of the heart itself, and of the murmurs which accompany its movements. The variations in question may show themselves from beat to beat. One beat may have a short murmur, evidently presystolic; the next may be preceded by a prolonged bruit, which may occupy nearly the whole pause, and might thus excite doubts as to its nature in the mind of an unpractised auscultator. In other cases the heart is apt to change its rhythm from time to time suddenly and without apparent cause. And thus whereas during one minute each beat is preceded by a most obvious presystolic bruit, the next minute no such bruit may be discoverable. Further on in my paper I shall have to discuss this point in considerable detail. I refer to it now, because several writers have most justly laid considerable stress upon variability of the heart's rhythm as a point in the diagnosis of contraction of the mitral orifice.

Another character which is observed to belong to many presystolic bruits is that of being brought out, or greatly increased when the patient is made to undergo even moderate exertion. I have more than once found that a direct mitral bruit, which was inaudible while the patient was sitting by the side of the bed, was perfectly obvious on his or her return from walking once or twice briskly up and down the ward.

When a direct mitral murmur is so prolonged as to be in fact diastolic in rhythm, it sometimes seems as if it grew out of the second sound. At the base, the second sound is clear, or perhaps appears to be reduplicated. As the stethoscope is carried downwards the double sound acquires more and more the character of a murmur, until at the apex it becomes the harsh, churning bruit so characteristic of mitral contraction. At first sight the fact just mentioned might appear fatal to the presystolic theory; it is nevertheless indisputable. Friedreich has already described this transition of the reduplicated second sound into a presystolic murmur in cases of contracted mitral. I have myself noted it more than once, and my observations are

perfectly independent, since they were made at a time when I did not even know that a reduplicated second sound had been mentioned as one of the signs of this affection.

I have repeatedly been asked by students whether the prognosis in a case of contraction of the mitral orifice is more or less grave than in a case of any other valvular disease, and particularly than in one of "mitral regurgitation." This question, however, is one of extreme difficulty, and, indeed, can in my opinion receive no direct answer. I have already shown that the discovery of a presystolic bruit is usually made at a period remote from the patient's death, unless this be caused by some complication. But this must be taken as proving, not that contraction of the mitral orifice is comparatively unattended with danger to life, but that when dangerous it is usually obscure. Thus the prognosis of a a case in which a presystolic bruit is audible would by no means be the same thing as that of a case of contracted mitral orifice. Further, the rarity of a rheumatic history in the former class of cases shows that they constitute to some extent a group apart from the rest.

A yet more serious difficulty in answering the question I have just put concerns the so-called "regurgitant mitral disease." The cases included under this head are still further than those of "contraction" from forming a homogeneous class, having a clinical history of its own. The most definite group among them seems to be that in which the mischief commences by the rupture of one or more of the chordæ; and this, I believe, is a very fatal affection. In another large group the dilatation of the left ventricle has probably much more to do with the symptoms, and even with the physical signs, than the valvular insufficiency. And, again, there are the cases of ulcerative endocarditis, the clinical features of which (as was shown by Dr. Wilks in the last volume of these 'Reports') are mainly due to contamination of the blood with morbid materials.

We must conclude that presystolic murmurs were formerly generally taken for systolic. But this is not, I believe, the only mistake which was made in such cases. I cannot but think that the first sound was at the same time supposed to be the second sound. In the early part of this paper I have shown how very easily this error may be committed, and how constantly it is in fact committed by students and others; the presystolic murmur indeed bearing to the first sound very much the relation which a systolic murmur would bear to the second sound. I have since found that both Dr. Gairdner and Dr. Sanders have drawn attention to the circumstance that in these cases the first sound is apt to resemble a second sound very closely. Dr. Markham, too, in his paper on the "Diastolic Mitral Murmur" already referred to, says that he has on more

than one occasion seen experienced observers mistake the first for the heart's second sound, when the action of the organ was great.

But further:—I am very much inclined to suspect that even now many of those observers who correctly diagnose the presystolic murmur in a case of contracted mitral orifice do nevertheless mistake the first sound for the second sound, believing the first sound to occur at the end of, and to be swallowed up in, the murmur, whereas it is in truth separate from it. Thus, in one of the cases already referred to, in which an autopsy verified the diagnosis, we find it recorded that "the first sound of the heart was so exceedingly feeble that it was very difficult to say in what relation this murmur (diagonised as auricular-systolic) stood to the first sound of the heart. The loudly marked second sound, however, was readily heard, and immediately afterwards the rough prolonged murmur." It may appear a very bold assertion, but I cannot refrain from asserting that the sound described as the second sound in this quotation was really the first sound of the heart.

This point is of the more importance, as it appears to me to afford an explanation of the very great discrepancies which are met with in the different papers on this subject, with regard to the difficulty of recognising the presystolic brut. There are some observers who maintain that it is "the most easily detected of all the cardiac murmurs;" there are others who regard the diagnosis of a presystolic from a systolic ventricular murmur as "one of the most difficult tasks in the physical examination of the heart, and as often all but impossible." The discrepancies will be at once accounted for if we suppose that the observers last referred to have been endeavouring to disentangle the murmur from an imaginary first sound, which they conceived to be blended with it, and to have mistaken the real first sound, for the second sound.

In conclusion, I cannot refrain from pointing out how important a bearing the facts detailed in this paper seem to have on that physiological theory which ascribes the first sound entirely to the tension of the auriculo-ventricular valves, and which, from its simplicity, appears at first sight so attractive. On the one hand, we find that in cases of extreme mitral contraction, in which the valve is thick, hard, and almost immovable, the first sound is peculiarly short, sharp, and clear. On the other hand, we observe that the auricular systole, at a time when no valve whatever is being stretched, is capable of giving rise to a sound scarcely differing at all from the natural first sound. Are not these facts almost conclusive in favour of the view that the tension of the ventricular wall is at least as much concerned as the tension of the valve in the formation of the first sound?—*Guy's Hospital Reports, vol.* xvi., 1870-71, *p.* 247.

## 20.—OBSERVATIONS ON PALPITATION OF THE HEART AND ITS TREATMENT.

By Dr. FREDERICK B. NUNNELEY, Assistant Physician to the Hospital for Diseases of the Chest, Victoria Park, and to the Hospital for Sick Children.

The frequency of the disease, and the disproportion often existing between the sensation complained of and the physical signs of disordered action of the heart, have led to the following remarks, in which the term palpitation refers only to the patient's sensation, and is thus used with its popular, though incorrect, signification.

Palpitation is one of those symptoms of a deviation from health, regarded by the subject of it with anxiety, or even with alarm, but of the nature and seat of the cause of which we only possess a slight knowledge, in so far as this consists in a structural alteration of some part of the nervous system.

A further difficulty arises from the fact that palpitation, disorderly or perverted action of the heart, and structural changes of the organ, are not always quite clearly distinguished from one another, and that a definite relationship between structural change or disorderly movement on the one hand, and palpitation on the other, is *necessarily* supposed to exist, whilst there is often no proportion between the inconvenience and pain which the patient suffers, and the signs of perverted action which can be detected by physical examination. The patient has the unnatural and uneasy cognisance of the action of his heart, and from this he seeks release, and is not unfrequently grateful for the cure of a disease of which he only knew the existence; in other words, a "beating of the heart" may be complained of when no deviation from its natural action can be detected; or there may be grave structural disease, and the most disorderly action, and yet the patient may make little or no complaint.

It is quite true that in many cases the heart *throbs* in a manner evident to the observer as well as to the patient; but in a considerable number the patient only can assert such to be the case. Palpitation is so often a mere symptom which, like pain, though quite real, varies in intensity with the individual, that it may be practically regarded as the *subjective appreciation of the heart's action.* Just as a patient who suffers pain after food—"knows that he has a stomach"—is the subject of dyspepsia, so a patient who feels the beating of his heart—"knows that he has a heart"—suffers from palpitation.

In health the heart beats with a frequency varying within narrow limits with each individual, the contractions are performed with regularity, and in definite order, best expressed by

the term rhythmical. Any deviation from this state is a *derangement* of the heart's action, and affects either the frequency, order, duration, or intensity of its movements, and the knowledge of such alteration on the part of the subject of it, or even of the normal action, constitutes *palpitation*. Changes in point of frequency and of intensity, whether real or apparent, are more usually associated with sensation than those of order or duration. so that in a case of palpitation, if any physical signs exist, there will probably be increased frequency and force of the heart's contractions.

The pathology of palpitation, as of perverted movement, is at present little known. There is no doubt, however, that it is to be studied in the altered structure of the nervous ganglia and plexuses connected with the heart, the centres of the vagi and sympathetic nerves, and the base of the brain and the spinal cord.

The exciting causes of palpitations are the various attitudes of mental and bodily excitement; overwork of the heart, as in severe physical labour; mechanical displacement of the organ; certain conditions of the blood, whether naturally acquired or due to the introduction of substances from without; inflammation of the heart; reflex causes, as impressions on other organs having a nervous connection with the heart; and also certain conditions which, like those giving rise to attacks of neuralgic tic, are for the most part at present unknown.

A scientific classification of cases of palpitation is not at present possible from our slender knowledge of its pathology, and from the want of proportion between the disease and its physical signs. The following arrangement is simply one of convenience, as it throws together cases more or less resembling one another in their clinical history and progress.

(*a*) Cases occurring in persons free from structural disease of the heart, and of any organ having a nervous connection with it. Two classes of these patients are met with—the young in whom degenerative changes have not commenced, and those in middle or advanced life in whom they have made considerable progress.

1. Palpitation occurs in young persons of both sexes, but especially in women, in whom it is often associated with pleurodynia. A mild form of irritative dyspepsia, and paleness not amounting to anæmia, are sometimes present. There is a history of impaired health, often dating from the time of the commencement of the catamenia, or of some moral shock in the nature of an affliction or disappointment, and a "beating at the heart" is complained of on the least exertion or excitement. The touch detects increased force of the heart's impulse; to the ear there is augmented intensity or abruptness of the sounds,

and their frequency often exceeds that of health. Usually there is no irregularity, but now and then there is an intermission, or one or other sound is reduplicated. In these cases there would appear to be exalted susceptibility of the heart to emotional impressions, or to trifling demands of extra work, such as is required in going up stairs, together with impaired nutrition of its nerves.

2. Palpitation occurring in middle or advanced life, apart from other diseases, is probably one expression of that degeneration affecting the nervous system which is at this period of life making progressive inroads on the integrity of every structure of the body. The clinical history of such cases accords with this view. The palpitation is often of long standing, and has come on gradually, appearing after some serious illness, recovery from which has been slow, or is ascribed to some heavy affliction which has impaired the health. It is more common in women than in men, and sometimes dates from the cessation of the catamenia. The least exertion or excitement brings it on; it often disturbs sleep, is essentially chronic, and is apt to recur after apparent cure. Physical examination reveals a weak diffused impulse, irregular in force and frequency, and often with intermissions; or it may be that the action is regular, but somewhat abrupt. Usually, however, the regular, abrupt impulse of earlier life is absent. These patients are often the subjects of dyspepsia, due probably to a degeneration of some of the glandular structures of the stomach, but the palpitation will persist long after the digestion has been fairly restored by the adaptation of diet and exercise to the lessened power of the stomach.

(*b*) Cases of *distinctly* reflex origin. These are few in number, and are chiefly the results of the ingestion of food improper in quantity or quality, removal of which gives speedy relief. Probably, as our knowledge increases, this class will be largely augmented.

(*c*) Palpitation, associated with structural disease of the heart or its valves, occurs at all ages. It is not a prominent symptom of these diseases (except of fatty degeneration), in the ordinary use of the term, which includes changes of the valves, causing murmurs, obvious hypertrophy, or dilatation sufficient to lead to the grave consequences of over-filling of the venous system, and appears to be rather the effect of impaired nutrition of the heart and its nerves than of the mechanical faults of the organ. Its characters are those of the palpitation of middle life previously considered, except that it is, perhaps, less closely connected with excitement or exertion, and is often not so amenable to treatment. An abrupt throbbing impulse is more frequently absent than present.

(*d*) Palpitation is sometimes due to mechanical displacements of the heart, as by pleuritic fluid, pericardial effusion, and tumours of the chest and abdomen. The patient feels the beating of his heart; but physical evidence of deranged action is very frequently absent.

(*e*) Palpitation occurs as a prominent symptom in certain conditions of the blood, as in simple anæmia, leucocythæmia, in gout, and especially in exophthalmic goitre. It is also occasioned by the presence in the blood of substances introduced from without, as in the case of tobacco-smoking and the use of strong tea.

(*f*) The cases comprised in this group are characterised by the occurrence of palpitation in definite attacks of sudden access, and by the proportion which is preserved between the perversion of the heart's action and the patient's sensations; and also by the evidence of the disturbed innervation of organs connected with the heart. They form the transition, as it were, from palpitation to angina pectoris, and present some analogy to tic, both of the neuralgic and painless kinds. The attack commences often without obvious cause; the heart's contractions are greatly increased in frequency, intensity, and abruptness; they may be regular and violent, or exhibit the extreme of disorder. The pain may vary from slight soreness and uneasiness at the præcordia to extreme agony, associated with the impression of impending death; and a sick and faint feeling with one may, with another, become repeated vomiting and actual syncope. Griping pains in the abdomen and diarrhœa are sometimes present. Such attacks are met with in gout, exophthalmic goitre, rheumatic inflammation of the heart and its membranes, in cyanosis, and sometimes when there is no discoverable disease. That they are neuroses of the heart is rendered probable by their analogy with tic just referred to, and with those forms of neuralgia of sudden access; and this is especially the case when no changes in the heart can be detected. The very frequent, abrupt, regular action of the heart seen in an animal, as the dog, after section of the pneumogastric-nerves, recalls such cases to the mind, and prompts the question whether they may not be due to temporary and partial suspension of the functions of these nerves.—*Lancet, Feb.* 18, 1871, *p.* 228.

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## 21.—ON ANEURISM AND DISEASES OF THE HEART AT ALDERSHOT.

By Inspector-General LAWSON.

Rokitansky was of opinion that the first step in the production of aneurism was the disease of the inner coat of arteries commonly designated atheroma in this country, though conti-

nental pathologists seem to confine that designation to this affection in a subsequent stage, when its deeper layers have undergone fatty degeneration, and the whole begins to break up. This, he says, gives rise to consecutive disease of the circular fibrous coat and cellular sheath (the term circular fibrous coat indicates the middle and cellular sheath the outer coat of the artery), and dilatation begins, sometimes embracing the whole circumference of the vessel, constituting the true aneurism, or proceeding from a limited portion of its walls, and expanding into a pouch of variable size, constituting the saccular form. A secondary dilatation sometimes proceeds from a true aneurism, which assumes the saccular character. (Pathological Anatomy, Sydenham Society edition, Vol. IV., p. 276-8).

With reference to saccular aneurism, he makes the following distinctions:—

"1. In most cases, the wall of the sac at its base near the opening into the artery, consists of the wall of the artery that has been everted by the aneurism, and of all the diseased coats of the artery, whilst the circular margin surrounding the opening is formed by a duplication, as it were, of the entire wall of the vessel."—"The margins of the opening are smooth and covered by the deposit; the aperture is roundish."—"This form of aneurism almost invariably occurs as a secondary formation, being seated on a cylindroid or fusiform aneurism.

"2. In some few cases the circular fibrous coat terminates sharply at the margin of the aperture in the artery. The aperture is irregular and angularly contracted, whilst the wall of the pouch above it consists of the cellular sheath, and of a deposit which projects from the contiguous inner surface of the vessel over the margin of the circular fibrous coat in the form of bridge-like plates and strings, and adheres loosely to the cellular sheath in the cavity of the expanded portion of the vessel. This appearance is observed only in small aneurisms, before they exceed the size of a bean or a hazel nut, and they commonly prove fatal by bursting. They usually occur as primary aneurisms, and in general in arteries that are only slightly and locally diseased. The aperture in the fibrous coat is obviously the result of loss of substance."

"3. In cases of similarly rare occurrence, we meet with a sharply defined bulging of the artery, filled with the atheromatous mass resulting from the disintegration of the deposit and the circular fibrous sheath. The wall is here composed of the cellular sheath."

Rokitansky considers the aneurism described under the first head "to be the result of excessive disease of the coats of the artery at a circumscribed spot. The artery bulges, and its walls then bend at the margin of the diseased tissue towards

the tube of the vessel with which it forms, as it were, a duplication of the wall of the artery." Further, he says:—"The appearances considered under the second head, have undoubtedly been developed from those noticed under the third head; at all events we are unable to discover in what manner this loss of substance has taken place within the wall of the vessel, unless by the atheromatous process described under the third head. We find that the deposit and the circular fibrous coat are affected throughout and destroyed."

He continues, "We take the present opportunity of answering the question whether a rent in both the inner coats of the artery can give rise to the formation of an aneurism of this class? The belief in this mode of origin has met with almost universal accordance, although, as far as we know, the correctness of the opinion has never been proved by any one. The cavity in the neck or pedicle of these last named aneurisms has commonly been regarded as a fissure. Yet, as far as we are aware, no such rent has ever been detected, nor have we ever found that a fissure in the inner coats of the artery afforded a basis for the formation of an aneurism."

I have been thus particular in giving Rokitansky's views, as no one would differ from him unless upon the clearest evidence, and I think this evidence is afforded by Clegg's case, detailed in the Sanitary Report of the Army for 1866, p. 552. In that instance there had been recent pericarditis, accompanied or followed by thickening in the coats of the right side of the ascending portion of the aorta within the pericardium, in the centre of which was a cavity, apparently of an abscess. This communicated with the canal of the artery by radiating fissures extending through the middle and inner coats, with abrupt edges, and at a point where there was no trace of atheromatous degeneration on the inner coat. The cavity of the abscess in this case had constituted the sac, which gave way while it was yet of very small size, but had the pericardium been adherent to the surface of the aorta at the part, the sac would have been sufficiently supported to have withstood rupture so early, and have attained a much larger size, as was found in several instances here during the year. Had Rokitansky met with a case resembling Clegg's, I cannot but think he would have modified his views as to the immediate cause of aneurism materially, and have given much less weight to atheroma than he has done. It would be going too far, perhaps, to discard the influence of this degeneration altogether, but certainly aneurism, as I have met with it among soldiers, seems frequently to exist quite independent of that form of disease of the inner coat of the arteries, and destruction of large portions of even the whole three coats may take place by an acute process, and without a

trace of atheroma in the neighbourhood, as is clear from the following case:—

Private John Almond, 80th Regiment, aged 28, 10 years' service, was admitted to hospital on 8th April with inflamed tonsils, accompanied by considerable febrile excitement. The left was most affected. Matter formed in it, and on the 11th was evacuated by a natural opening in the anterior part of the tonsil, after which the febrile disturbance disappeared. On 15th at 6.30 A.M., while getting up, he was in high spirits, and, while talking to those about him, a gush of blood came from his mouth; this was florid, and a large quantity escaped, causing faintness. Cold water was applied to the chest and spine, and ice given in small quantities frequently during the day. There was no recurrence of bleeding until the morning of the 16th, when he got up contrary to order, and it returned, and he lost nearly a pint, which came away in gushes, not by vomiting or coughing. Perfect quiet was now maintained, and ice kept constantly in the mouth, and he went on favourably until the 21st, when at 3.50 P.M. a violent gush took place, and though pressure was made over the carotid immediately, he died in five or six minutes.

The last attack was preceded for a little by a peculiar sensation in the region of the heart, which induced the patient to think the bleeding was about to recur. On examination the cavity of an abscess was found behind the superior constrictor of the pharynx, about the size of a filbert; this had opened into the pharynx behind the velum by a small aperture; on the other side it communicated with the canal of the internal carotid, the coats of which were destroyed for nearly half an inch in length, and about a fifth of its circumference in width, at a point just before it turns to enter the carotid foramen of the temporal bone. The edges of the opening in the carotid were irregular, abrupt, and thickened as if they had undergone inflammation and ulceration. From what we know of the resistance of the coats of the larger arteries to ulceration, even when passing across an eroding sore or abscess, it appears reasonable to conclude that the vessel in this case was itself originally implicated, and that the resulting abscess opened through the muscle, and not that an abscess first arose there which ultimately led to the destruction of the coats of the vessel. It was a question whether the common carotid should be tied in this case; the difficulty in finding where the blood came from, and its intermittent flow, however, led to its not being done, and after death it became clear that even had the operation been performed, the bleeding most likely would have recurred, owing to the free anastomosis of the vessels inside the cranium, affording a copious supply of blood, which the large breach in the

carotid would have permitted it to escape on the first excitement.

In the following case there were two aneurisms, which clearly came under Rokitansky's second division, at the same time that there was thickening of the whole circumference of the vessel, and several commencing dilatations that would come under his first division, but without atheromatous degeneration at the spot.

Private Edwin Fowler, Military Train, aged 31, service nine years, healthy in appearance, steady, temperate, and with great muscular development, went to hospital on 18th November complaining of pain between the scapulæ, which had been increasing for some days, but without cough or acceleration of pulse. Nothing unusual was discovered in the condition of the lungs, heart, or great vessels, nor was there any tenderness in the course of the spine. About 2 P.M. he become rather collapsed, with great uneasiness deep in chest, which gradually extended downwards, and at last was referred to the epigastrium. This was attended with much restlessness, and inability to remain in any particular position; breathing deep and sighing, pulse small and slightly accelerated, and latterly there was a feeling of sickness at stomach, and a sensation as if the bowels were to be moved, but nothing was passed either way. In this condition he died at 4 P.M. On examining the body the coats of the aorta at the commencement of the descending portion were found thickened for between two and three inches, and this embraced its whole circumference. On laying open the vessel at this place, two small aneurismal pouches were found about its centre, and at the inner and back part of the vessel. These were near each other, and had openings about the third of an inch in diameter, with smooth but mamillated edges. The smaller sac was about half an inch deep with its inner surface entire. The other, which could have contained a walnut, adhered to the vertebra at the back, and had given way at the lower part, and permitted extensive effusion of blood into the posterior mediastinum, both upwards, and downwards as far as the diaphragm. At several other points inside the thickened portion were slight, nearly circular, depressions of the surface, showing the resistance of the coats to the pressure from within had been insufficient, and that other pouches were commencing to form. The lining membrane of the vessel displayed moderate traces of atheromatous degeneration, many patches of which had become perforated with numerous openings, as is usual when they are wasting, but the depressions of surface above noticed were free from atheroma in any of its stages. The heart was healthy in size and appearance. The tricuspid and mitral valves were quite healthy. The aortic had slight thickening towards

the edges, but were smooth and healthy in appearance otherwise.

This case shows that the inflammation which precedes the formation of aneurism may embrace the whole circumference of the vessel, and hence affords an explanation of those varieties of the disease denominated true aneurism, in which the calibre of the vessel is suddenly increased without the formation of a distinct pouch on one side, communicating with the canal of the vessel by an opening more or less contracted. Three such examples were met with among the fatal cases in the course of the year; in one of them, Gunner Martin Henley, Royal Horse Artillery, who died on 7th September, the immediate cause being pleuro-pneumonia of left side, the aorta was dilated into a large pouch without any clot in it, and without any distinct sac. In the second, that of Private J. Miller, Military Train, who was also affected with disease of, and incompatency of the mitral and aortic valves, the aorta, after giving off the left subclavian, suddenly expanded into a pouch about 2½ inches in diameter, and circumscribed portions of the walls of this again were commencing to be dilated so as to present a number of secondary pouches; there was no trace of fibrinous deposition in any of these. There was very little trace of atheromatous degeneration in this case; the immediate cause of death was congestion of the lungs. In the third case, that of Private Blake, 98th Regiment, the aorta, about the commencement of the arch, was dilated uniformly to the size of a cricket ball, the inner surface smooth, and without any deposit of fibrine. There was much atheromatous deposition between the heart and sac, some of it calcified. The tricuspid valves were much thickened and incompetent, also the aortic, and the heart enlarged. The case proved fatal through congestion of the lungs and dropsy. The man's condition had been detected in India when he presented himself as a volunteer, and to the last he stated he felt no inconvenience from it.

Occasionally an instance is met with of a distinct aneurismal sac, combined with a general dilatation of the vessel at another part in the vicinity, as in the following case, which presented both features, and with a complete absence of the atheromatous deposit.

Private Joseph Abbot, 54th Regiment, aged 32 years, ten years' service, died of aneurism on 18th June. This had made its appearance a considerable time before, but being on Staff employ he did not report himself until 10th May, when it had caused absorption of the sternal ends of the second and third ribs on right side, and protruded externally. While in hospital the tumour extended up the neck as far as the thyroid cartilage, and ultimately caused absorption of the clavicle. The skin over the upper part of the tumour became thinned, inflamed,

and threatened to ulcerate, when he died from exhaustion. The sac arose from the superior portion of the arch of aorta, by a large opening; it was nearly filled with concentric, and firm layers of lymph; it had given way at one point, and from this the blood escaped which had extended through the areolar tissue along the neck, here there was no attempt at fibrinous deposition. The innominata was pervious and healthy, the left carotid obliterated at its origin. The aorta was much dilated between the heart and the sac, and its inner coats puckered, thickened and rough, but presented no trace of atheroma. The heart and valves were healthy.

In addition to the cases already alluded to, there were four in which sacs of some size had formed within the pericardium, and ruptured into the cavity, in all of which this membrane was adhering firmly over most of the surface of the sac; in one of these there was a second sac outside the pericardium. In another case, that of Private Neighbour, 97th Regiment, who died from exhaustion without rupture, a large sac sprang from the anterior part of the arch of the aorta, and another, the size of a walnut, was formed between the aorta and pulmonary arteries and upper part of the right auricle. It communicated with the aorta by a smooth circular opening that would admit a swan quill, half an inch above the valves. The sac was firm, and contained many layers of fibrine which almost filled its cavity, it was, in short, undergoing a spontaneous cure. Another case had a sac at the commencement of the arch, which burst into the right pleura. In another there was a large sac at the arch which did not give way, but the patient died of exhaustion. In another, a sac at the end of the arch ulcerated into the œsophagus, and after death the stomach and duodenum were found distended with blood, though none had been passed from the mouth. In another there were two sacs at the commencement of the descending aorta, one of which burst into the posterior mediastinum, the blood passed downwards as far as the crura of the diaphragm, and then into the right pleura, which contained numerous large clots. In another there was aneurism involving the cœliac axis, which burst into the peritoneum. Of these ten cases there was no atheromatous degeneration in the inner coats of the aorta in three, and in one only were the aortic valves diseased. In five others the condition of the inner coat was not specially noticed; two of these only had disease of aortic valves. In two there was much atheroma, but in both the aortic valves were healthy.

There was one case in which an aneurism arose from the bottom of the sulcus behind the anterior aortic valve, and the sac was formed in the left ventricle, by the endocardium with a few muscular fibres adhering to it, and the upper part of the septum.

The cause of death in this case was peculiar, and as there was another presenting much the same characters, but without complication with aneurism, it may be useful to bring both together here.

Private Thomas Pope, 1st Battalion, 20th Regiment, aged 27, service eight years, was orderly to Assistant Quartermaster-General since 10th June. He complained latterly his duties exposed him to the sun, and were harassing. For a short time before his death he seems to have felt much uneasiness in cardiac region, and to have dreaded ascending the hill from the east Infantry block to the Quartermaster-General's Office (about 70 feet elevation). On the evening of 29th February he complained of pain in the region of the heart with palpitation, but next morning took his breakfast heartily, and went to the office. At the usual hour he dined, and shortly before 2 P.M. was sent a message, when he fell down, but immediately after was able to walk to hospital with the assistance of a comrade. At half-past 2 P.M., Dr. Carter saw him, when he complained of pain in cardiac region, with great difficulty of respiration, with slight cough, and much moist crepitation over lungs. Pulse very feeble, temperature of surface natural. At a quarter to 3 P.M. the breathing was much more laborious, and there was profuse expectoration of frothy fluid, slightly tinged with blood. At 3 P.M. he was insensible, collapsed, pulse scarcely to be felt, and the froth oozed up into the mouth, and passed out of the nostrils in very large quantities; he had previously spat a quart into the vessel at the bedside. Sinapisms were applied to the chest, and ether, camphor, and brandy given freely, and latterly Silvester's method of restoring the drowned was employed, but without effect, and he died at 3.35 P.M. The fluid expectorated was alkaline with a number of blood corpuscles in it. It consolidated with nitric acid. Hydrochloric acid was not tried with it. As already stated, an aneurismal sac was found at the upper part of the septum in the left ventricle, this was about the size of a walnut, and had no deposit of lymph in it. When full the sac could not have been emptied during the period of contraction of the ventricle, and as it was protruding over the passage to the aorta, must have impeded the flow of blood into that vessel very materially. The valves and lining membrane of the aorta were healthy. The lungs were much congested, almost hepatized, and the bronchial tubes full of fluid.

The other case was that of Private A. Holdgate, 54th Regiment, which was returned as pulmonary apoplexy. This man, aged 32, was stout, muscular, short necked, and had 14 years' service. On 31st July, after dinner, while employed on fatigue duty, he felt unwell suddenly, and was removed to hospital on a stretcher at 4 P.M. He was then in a low prostrate condition,

with irregular intermittent pulse, and continued in this state until about 8 P.M., when cough, dyspnœa, and frothy expectoration came on, and the latter soon presented an admixture of blood. By 11 P.M. the chamber pot was half filled with a thin serous-like fluid, frothed on the surface like soap suds. As this coagulated firmly with nitric and with hydrochloric acids, it was evidently chiefly serum, with but little mucus in it. The dyspnœa at 1 A.M. was extreme, with great thirst, the stomach rejecting everything as soon as swallowed. Pulse 120, temperature 56. Shortly after three the cough and expectoration gradually ceased. At 7 A.M. he became insensible, and died about three quarters of an hour afterwards. At first a mustard emetic was given, and a large sinapism applied to the chest, and ammonia and ether frequently administered. After the cough commenced turpentine fomentations to back and front, with the stimulants and brandy, were employed assiduously, and warmth applied to the feet. The body was examined 10 hours after, when the rigor mortis was well marked, and there was considerable livor of the back (depending) parts. White froth issued freely from the mouth and nostrils, and on compressing the thorax a continuous stream welled up. On incising the skin the veins bled freely. The heart was somewhat hypertrophied and dilated, valves healthy, walls with a good deal of fat. Right side full of fluid blood. Left ventricle contained a feebly coloured clot attached to its anterior wall. Right pleura firmly adherent throughout. Trachea and bronchial tubes full of frothy serum. Lungs, especially, right, much congested, but every portion floated in water. No other indication of disease in them.

The symptoms in the first of these cases might have been attributed to the impediment to the circulation caused by the aneurism, but for the immediate occurrence of the other in which no such obstruction existed; it seems very probable, however, that the extremely rapid course of the disease in Pope was determined by this complication. The weather had been very hot up to 22nd July; on 23rd the maximum was 72·4° only, and it was not above 80° for the rest of the month, except on 27th and 28th, when it rose to 89·4 and 85·4 respectively, but on 29th was 72·8 only. The excessive discharge of serous fluid from the lungs bears some analogy to what is described as occurring occasionally in sun-stroke, but (indepenently of the cases occurring after the temperature was much reduced) it was not preceded or accompanied by the dryness and excessive heat of skin, or by the other symptoms characteristic of that disease. There was a disposition to cholera at the time these cases presented themselves, and, on 13th August, a man of the 2nd Battalion 6th Regiment actually died of that disease.

I am not aware that a connexion can be established between them, but in the absence of any trustworthy explanation of the causes of the former the coincidence may be mentioned.

There was an instance of aneurism of the pulmonary artery, a very are form of disease, and the first I have heard of in the Army.

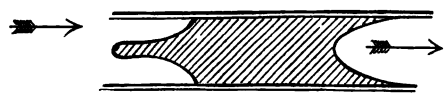
This occurred in Schoolmaster Serjeant James Thane, 98th Regiment, aged 27, who had complained of hoarseness and occasional dyspnœa for about six months before death, but for which no perceptible cause had been detected. On 2nd September he was found dead in bed, his head hanging over the side, and a large pool of blood on the floor that seemed to have flowed from the mouth and nose. An aneurism of the pulmonary artery, the size of a pigeon's egg, was found, which had burst into the left bronchus, by a circular opening two lines in diameter. The sac contained dense layers of fibrine. The bronchial tubes on the same side were filled with blood, but the lungs were otherwise healthy, and there was no valvular disease of the heart.

There were two cases of popliteal aneurism under treatment, which were cured by pressure, and both men have since returned to duty. The subject of one of them was Troop Serjeant-Major Henry Wadeson, 10th Hussars, aged 30, and with 13 years' service, a stout healthy looking man. His horse fell with him at the Curragh on 6th May. Immediately after he marched for Aldershot, and on 12th he felt a sharp pain in left ham, with stiffness of limb, but he continued with his troop until he reached this station. On 7th he came to hospital, with a well defined aneurism as large as a hen's egg; pressure was applied to the femoral artery in front of the thigh, and the pulsation ceased in 70 hours. The limb remained stiff and weak for some months, but he has latterly been able to resume his mounted duties.

The other case of popliteal aneurism occurred in Private J. Dillon, 97th Regiment, aged 30, and 10 years' service, eight of which had been in India. This man was healthy in appearance, but of intemperate habits. He was admitted on 8th November with aneurism in left ham the size of a hen's egg; he had experienced stiffness, numbness, and cramps in the leg for a fortnight before, but was not aware of having injured it in any way. Pressure was applied over the femoral artery in front of thigh, and continued with intermissions until the 20th, when the pulsation ceased, and the swelling then diminished rapidly. The man was discharged to duty on 8th December, and has continued at it since, unless when in confinement.

During the year there was an opportunity of examining the condition of the artery in a man who had had aneurism at the junction of external iliac and femoral on left side, some years

before, which had been cured by the pressure of a 4½ lb. weight over the femoral below its site, while on detachment in New Zealand. The subject, Private Charles Carlton, 43rd Regiment, died of dropsy, depending on tricuspid incompatency, on 27th March. The canal of the artery was completely obstructed under Poupart's ligament, for about 1¼ inches in length, by a firm fibrous material in this manner—



The coats of the vessel could not be separated from this plug on the one hand, nor from the sheath on the other, the whole being matted together by extremely firm fibrous tissue. There were no distinct remains of a sac perceptible.

From the above details I think it may be concluded that when the dilatation of the vessel is uniform, embracing all its coats, there is little or no chance of fibrinous deposition, and of a natural cure; but, on the other hand, unless secondary sacs form, the simple dilatation seems not disposed to extend indefinitely, and to involve risk to life, so that such cases, as instanced in Private Blake, 98th Regiment, may run on for a long period without the individual experiencing much inconvenience, and even allowing of his undergoing considerable exertion if the heart be not affected.

As soon as a distinct sac is formed, whether directly from the artery, or from a true aneurism, there is a tendency to the deposition of fibrine, and under favourable circumstances as to rest and diet, that will proceed quickly and may ultimately lead to a complete cure. In the case of Boddy, 54th Regiment, the deposition of fibrine was particularly rapid after he was placed in bed, and the size and pulsation of the tumour diminished, and, had the sac not given way so as to permit of extravasation along the neck there seemed every hope of a cure. The case of Neighbour, 98th, in whom a small sac, within the pericardium, was almost completely filled with dense fibrine, shows that even in that most dangerous situation a cure is not to be despaired of. Of course the facility for deposition, and the probability of a cure, will be greater when the opening into the sac is small than when it embraces a larger portion of the calibre of the vessel. When the arterial coats have given way by a rent, and the blood is effused into the neighbouring parts without the intervention of a sac, the chances of a cure, unless the artery itself be secured both above and below the part, are extremely small. Rokitansky indeed says, that inflammation of these sometimes ensues, leading to a deposition of fibrine

around the extravasation, and so forming a sort of sac, but this occurs so rarely that the probability of its taking place in any given instance cannot be regarded as of any practical value.—*Mr. Lawson's Pamphlet, p.* 6.

## DISEASES OF THE ORGANS OF RESPIRATION.

### 22.—ON THE VARIETIES OF TUBERCLE, AND THE RELATION OF TUBERCLE TO INFLAMMATION.

By Dr. W. MOXON, Assistant Physician and Pathologist to Guy's Hospital.

Although some tubercles are comparatively yellow and opaque, and some comparatively grey and pellucid, yet it is wholly futile and wrong to suppose that tubercles can be divided into yellow and grey; and to know this will greatly help us in considering the rights of the contest between those who have affirmed and denied their identity.

The question in discussion in reference to grey and yellow tubercle has been this: Are they only stages of the same thing, or are they distinct kinds of tubercle? Those who hold them to be stages, pointing to the fact that in the same lung you see yellow tubercles, and below these, grey tubercles with yellow centres, and yet below these, grey tubercles. They thought this showed that in spreading down the lung from above, the newer tubercles are found commencing grey, and the older ones are growing yellow in their central older point, while the yet older ones above these are quite turned yellow. On the other part, the holders of the belief that the grey and yellow tubercles are different in kind, are driven or drawn to this belief by the greater impression they receive from a fact equally true—namely, that in examining different cases you find in some the tubercles all to have a prevailing yellow colour, while in some other cases they have a prevailing grey colour,

Now, in asking this question there are really three distinct questions asked confusedly; these are—1. Does grey tubercle ever turn yellow? 2. Does grey tubercle ever not turn yellow? 3. Does yellow tubercle ever appear without being grey at first? Look carefully at each of these questions. An affirmative answer must be given to each. No doubt grey tubercle does turn yellow at its centre. No doubt grey tubercle does not sometimes so turn yellow. And no doubt tubercle is sometimes yellow from the first.

To understand how this is, we must see that there is another element, which those who treat the question as an alternative between stage and kind leave out of consideration. This ele-

ment is the element of *intensity*. A yellow tubercle may be an older tubercle than grey, or a different kind from grey; or it may be a *more intense tubercle than grey*. Let me dwell on these points a little more. I stated that the question asked in reference to these celebrated two forms of tubercle is practically this question: Is the grey a stage of the yellow, or is it a different kind?

Now, I want you to see plainly that there is yet another sort of difference, besides these of stage and kind, and that sort of difference is—difference in degree. We said that grey does soften into yellow, and that it does not soften into yellow; and that the yellow comes without it. Now, the first of these propositions would argue an identity of kind, but the second and third appear at first to militate against the supposition of identity in kind. Are they really opposed to the supposition of identity in kind? No; because the absence of a grey stage is due to high intensity, the presence of a grey stage to low intensity of the tubercle—a lower or higher degree, not a different kind; just as in inflammatory lymph we get a plastic, pellucid, greyish matter, called "plastic lymph," or a yellowish, opaque, solid or liquid substance, called "puriform lymph," or pus. We know in the case of lymph and pus that their difference is one of intensity, because we are able ourselves experimentally to determine the production of one or other by graduating the intensity of cause. If you draw a thread soaked in croton oil through the testis of a dog, you get the production of pus, and some gangrene perhaps; if, instead of croton oil, you use a thread either clean or with some less irritating substance on it, you get a less and less degree of inflammation, according as it is a less and less irritating substance that you employ. And hence you recognise purulent and gangrenous as the extreme severity of inflammation, and the plastic as a lower degree of the same. Or, if you proceed another way in the judgment of these relations, you will find, on examining, that at the focus of irritation you get the production of pus, while a grey plastic lymph is produced further from the focus; so that, when the pus is fully formed, the grey plastic matter organises to form a sac, and encloses the pus.

These facts prove beyond doubt that the plastic lymph in inflammation is the same thing as, but lower in intensity of causation than, the pus. Now, we are not able to cause tubercles in this way directly by graduated degrees of causation, but analogy will, I think, convince us that the yellow and more pus-like tubercle is an intenser one than the grey or more lymph-like tubercle. And there is more than analogy in support of this, for the severer and more rapid the tuberculisation of the patient, the more yellow is the tubercle found to be.

And again, on quite another hand, the yellow tubercle is destructive of the substance it occupies to a greater extent than the grey, in which you generally still see remains of the tissue; while again, further, the elements of the grey tubercle, like those of plastic lymph, show more or less tendency to elongate and organise. If we are right in using this simple analogy, and rightly use it, it clears a way to remove all the difficulty.

But if it be found that a yellowness supervenes on the grey tubercle as a result of oldness, and that, on the other hand, a severity of intensity produces a yellowness in the tubercle not preceded by greyness, it may be fairly asked—Is the yellowness which comes from oldness of the grey, the same yellowness as that caused by severity in the originally yellow tubercle? This is a question of fact to be settled by observation. You will find, I believe, after examining specimens very carefully, that the yellowness of grey tubercle is a fatty degeneration of, and a breaking up of, the elements of grey tubercle. These elements you will find to be not quite the same as those of the originally yellow tubercle; they are often elongated and tending to form fibres, which may unite together and produce true tissue, so that some grey tubercles in this way reach a healed condition, the tissue so produced assimilating in the lung to the nature of pulmonary fibre, which is elastic. The little knot becomes horny, and the tubercle is then said to be "cornified." This drawing shows the result of such cornification. The greyer the tubercle—that is, the more lymph-like—the more chance of this occurrence. I have already said there are all degrees of greyness toward yellowness. Now, the yellow change in this grey plastic fibrillating tubercle produces fatty degeneration of these more fibrillar elements, at last breaking down to pulp, while the yellow tubercle is from the first composed of round cells with no plastic tendency to elongation, but very early becoming fatty. This tendency to fattiness in either case is the cause of the yellowness; but in the grey tubercle the supervening fattiness comes more slowly over its longer-shaped and better-lived plastic elements, while in the yellow it comes more swiftly over its round and pus-like aplastic elements, these never showing tendency to elongate, but being round and pus-like; yet even these elements are at first not fatty, and a small zone of grey colour can with care be always detected around the yellowest of tubercles. Thus, then, the yellowness of the late stage of grey, and the original yellowness of yellow tubercle (which we see is also a late stage, but so quickly reached as to be practically original), are the same yellowness, in that both are due to fatty degeneration and consequent opacity; but, in that the things that so change yellow are not quite the same in structure, the slowly faded grey tubercle has certain difference

from the quickly faded yellow; but, further, in that the difference between the slowly faded grey and originally yellow elements is one of intensity of cause, they are not different kinds of tubercle. So, I think, the truth seems to be this way —that grey becomes yellow tubercle, and yellow tubercle arises without grey (evidently) preceding; but the yellow tubercle which comes on upon the grey is not quite the same as the yellow tubercle that originates yellow. Yet the yellowness is of the same nature, in either being only fattiness. But the difference between them is made by the nature of the things which become yellow by fat: which things in the greyer tubercle are, on the average, more elongated, in the yellow tubercle more rounded, the elongation and roundness of element being in their turn parallel in nature to the like varieties of form in elements of common inflammatory lymph, and by analogy and by direct evidence shown to be, as in the case of lymph, the results of less or more intensity of action in the cause of the disease.

Pray observe that the difference in regard of yellowness is only one of degree; the tubercles that are apparently originally yellow always and invariably have a short stage of greyness, this stage not showing itself all through the tubercle at one time—for, little as it is, the tubercle is a spreading, growing thing—but showing itself in the outer zone, which is last produced and youngest, and which, as I said before, is always grey, however narrow it may be.

So much for the relation of grey and yellow tubercle; now as to the relation of this to inflammation. If you see only small tubercles in a lung, you are content to say they are tubercles, and leave out of account the question of inflammation. They are tubercles, and you would no more care to inquire whether they are, nevertheless, inflammation than you would care, having recognised a psoriasis, to ask whether it was an inflammation. It is a psoriasis. But in some cases you find that, though some tubercles are small knots of firmish consistence, you see in the same lung other patches of larger size, and others yet larger; so that from the little tubercles to the larger patches there are intermediate gradations. Now, the larger patches are granular, and sink in water, and are softish, breaking down easily when pressed, and exuding a yellow-looking opaque juice when so crushed down—and, in short, they have all the qualities which characterise the so-called grey and red hepatisations of the lung in acute pneumonia. Then arises a great difficulty—for the tubercles have pneumonia in such a relation that you cannot but see that the one is the cause of the other.

I say you have pneumonia with the tubercle; and, indeed, there is no doubt that the characters (in the cases which I am

referring to) and state of the larger masses are, in all essential points, the same as those in pneumonia arising from other causes.

Now, you cannot think the pneumonia indifferent to the tubercle: you must allow some relation between them; and these are the questions that arise—1. Does the tubercle *cause* the pneumonia, as one thing causes another and a different thing? 2. Does the tubercle *become* the pneumonia, by expansion of its area and diffusion of its influence over greater space in a less time?

Those who hold the first view will say the tubercle has *set up* a pneumonia. Those who hold the second view will very likely, when pressed, say that the patch of pneumonic-like formation is "*really tubercle*," a patch, not of pneumonia, but of soft "diffuse tubercle." Now, if these two views are allowed to conflict against each other, so that one is made to contradict the other, each makes the other absurd to its own satisfaction, and goes away overmuch satisfied with itself.

For, if we look disinterestedly at the question, we shall see that its solution lies here—*that there is no such thing as the pneumonia or common inflammation, which is so easily assumed to be at hand* by those who would hold the first view, and say that tubercles set up that simple pneumonia. All pneumonias have their habits; there is no common pneumonia which has no special habits. There is no simple pneumonia to which other pneumonias stand in such relations that their pecularities are additions to the characters of simple pneumonia, which simple pneumonia waits to apply itself around indifferent causes. The stock of simple pneumonia is plentiful in theory, but you do not find it in real lungs. In real lungs all pneumonias have their distinctive characters. *Every inflammation is intensely specific.* It is very much with it as with the tribe of cats. The lion and tiger are not common cats with additional lion and tiger peculiarities, any more than common cats are lions and tigers with common peculiarities. If we look over the pneumonias that occur in a year, we find that no pneumonia has the characters and distribution of the tuberculous pneumonia except tubercles be present with it—that is, *except it be a tuberculous pneumonia.* It is quite a peculiar thing, and its characters, so far as they vary from the characters of other pneumonias, vary towards the characters of tubercles, and in any lung vary indefinitely towards those characters, so that at last you find them to be tubercles. I shall be told that Magendie and others have put mercury into the lungs, and made artificial tubercles with surrounding common inflammation. But I beg to say that I could easily have seen that these were not tubercles, and am quite willing to challenge anyone to deceive me with such productions—they are only naked-eye-deceiving models of tuber-

cles. Besides, he never made anything like the expanded formations of pneumonic phthisis, of whose graduation into tubercle I have been speaking; nor can they be made. He produced the pneumonia of mercury's irritation, which is a different-looking pneumonia, and a different pneumonia from phthisical pneumonia.

In this discussion of the question, I have purposely avoided using the term inflammation. I have used the word pneumonia, because it will probably carry with it the signification of the produced matter due to the inflammatory change in the lung. From what I have already said on hypertrophy, you know what I should say if the question of inflammation is brought as an alternative to tubercle. I should say the question is an unmeaning question—it is not right to say a stupid question; it asks what cannot be answered, unless you mean by it —Did the phenomena of pain, heat, swelling, &c., which are the true and only meaning of inflammation, accompany the tubercles? This they certainly do sometimes; perhaps always. It is affirmed, I believe correctly, that *the formation* of tubercle always goes with an augmented heat of the body, ascertainable thermometrically.

The formation of a tubercle is as much inflammation as the formation of a herpes or a lichen, and as little; and as you get an inflamed lichen or psoriasis, so you may get an inflamed tubercle. In the case of the cutaneous disease, you would think anyone trifling with your time if he should ask whether the inflamed psoriasis is a psoriasis *plus* an inflammation; and so I think you should think of anyone who asks the question in reference to tubercle. It is as much tubercle as it was before when it is inflamed; but now it is so intense that the phenomena of pain, &c., the vital reaction to excessive change, are brought out along with the vascular phenomena which evince, characterise, and indeed create them—which vascular phenomena are the old and present and future import of the word "inflammation"—the subjects of the treatment directly addressed to inflammation, and the causes of such phenomena, additional to the cell-production of the tubercle, as are due to the vessel dilatation, blood stagnation, fluid oozings, &c., that the vascular reaction or inflammation brings to pass.

I will conclude this lecture with an analogy which, I hope, will serve to place the relations of the various formative diseases clearly. If you asked What is light? you would be told it is vibration. If you asked What is sound? you would be told it is vibration; and so, probably, scent is vibration, while some vibrations give a sense of touch. But though your mind sees all these as vibrations, yet your senses in a way analyse the vibrations, and your eye discerns the light of its vibrations,

and your ear the sound of its vibrations, and so on and on; so that your senses do a service that is in a way opposed to that of your mind, and in their way are cleverer, and certainly more practical, than your mind, which would never have found out that vibrations are light, however well it knows that light is vibrations. And no one ever did or ever will tell us what it is that, added to vibrations, makes them light, or sound, or scent. Now, it is in a very similar way that, when you ask What is *inflammatory formation?* you are told it is cell growth; and when you ask What is tubercle? you are told that it is cell growth; and so tumour, and a lot more. The viewing of these different things with sufficient eye-power by the microscope evidences this beyond doubt or question. The foundation of all these is one and the same thing—the production of nucleated cells, which grow when they are produced. This is common to them all, like vibrations to light and sound; yet the result of these growths is to produce here a tubercle, and here a patch of lymph, and there a tumour; and clinical experience—that is, the watching of the phenomenal results of the cell growth—evidences the differences between them. Clinical experience, as it were, further analyses the life of cells, as the senses analyse vibrations; but what is it that, added to cell growth, makes it become tubercle, or tumour, or lymph, you do not know, and never will know. It is very likely that your mind is not able to frame the question which anticipates the solution of the problem.—*Medical Times and Gazette, Jan.* 21, 1871, *p.* 64.

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## 23.—ON THE VALUE OF TINCTURE OF ARNICA IN THE TREATMENT OF ACUTE PULMONARY AFFECTIONS.

By C. C. BALDING, Esq., Shefford, Beds.

[The use of arnica internally as a remedy was suggested to the writer by an article published many years ago by Mr. Mitchell Henry, advocating its use in irritative traumatic fever. Mr. Balding first employed it in a case of crush of the chest between two railway buffers, and the marked effect it had in controlling the frequency of the pulse determined him to employ it more extensively.]

Bernard T., aged twenty years, a strong, muscular young man, a brickmaker, attended at my surgery, four miles from his own home, on Sept. 1st. He was seen by my assistant, Mr. P. P. Langford, who reported him suffering from acute febrile disturbance. He advised the patient to get home as quickly as he could and go to bed.

On the 3rd Mr. Langford visited him, and found that he was suffering from acute pleuro-pneumonia of the right side. Pos-

teriorly there was dulness over nearly the whole of the lung, with friction sound and some small crepitation in the upper part. At the base of the left lung, for about two fingers' breadth posteriorly, the mischief had commenced. Pulse 100, full and incompressible; respiration not taken. Mr. Langford told me of the case before prescribing, when I advised him to give ten-minim doses of tincture of arnica, with solution of acetate of ammonia, every three hours.

Sept. 4th. I visited him. His countenance was then dusky and anxious; pulse 100; respiration 60. The physical signs of the inflammation showed that it had not extended since the previous day. To continue the medicine.

5th. Seen by Mr. Langford. Pulse 85; respiration not taken. Medicine to be taken every four hours.

6th. Pulse 80; respiration 40; countenance improved. Continue medicine.

7th. Pulse 72; respiration 32; small crepitation audible in upper part of the lung posteriorly. To take the medicine every six hours.

8th. Pulse 60; respiration 24; lung improving. Continue medicine every six hours.

10th. Pulse 50, irregular; respiration 30. He was sitting up in bed, taking a milk mess, when I visited him, and expressed himself as quite comfortable. To take five minims of the arnica three times a day.

12th. Pulse 60, irregular; respiration 24; right lung normal, except for about two fingers' breadth at base, where there is small crepitation; left lung well. To discontinue medicine.

The rapid absorption of the effused products of inflammation in this case is remarkable, but it is only what I have seen in several others. The persistent effects of the drug also are very noticeable, as I have known the pulse remain at 40 for several days after the medicine has been discontinued, and even after the patient has got about. The case of hæmoptysis is briefly as follows:—

Mr. W. B., small farmer, aged sixty, came to me on the 18th of August last, and said he had coughed up, he thought, three pints of blood on the previous day. I examined his lung, and did not find any signs of extensive tubercular disease. He had been subject to winter bronchitis, and there were symptoms of an atheromatous state of vessels. Pulse 90, full and incompressible. Ordered ten minims of tincture of arnica, with fifteen of dilute sulphuric acid and syrup, every four hours. His pulse was very soon reduced, and in four or five days was down to 40, and remained so for three or four days; he got about his employment in ten days, and has had no return of the hæmoptysis.

I read a paper on the subject, three years since, before the South Midland Branch of the British Medical Association at Luton. Dr. Prior, of Bedford, then president, entered into the subject, and has since given the remedy a trial. At the last meeting of the Branch Association at Aylesbury, a paper of his, illustrated by cases, was read, which spoke very approvingly of the remedy.

I may state that I used to give but five-minim doses of the drug, but since the Pharmacopœia of 1867 has contained a formula for the tincture, which is evidently weaker than that previously in use, I give ten minims. I must ask those who may be induced to give arnica a trial, not to be dismayed should no benefit be apparent in twenty-four hours, but to continue the medicine, and I feel sure the effects will be unmistakable in forty-eight hours; but I have seldom seen a case really get worse from the time the arnica was commenced.—*Lancet, Dec.* 24, 1870, *p.* 885.

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## 24.—ON THE OPERATION OF TRACHEOTOMY IN THE LATER STAGES OF CROUP AND DIPHTHERIA.

By Dr. GEORGE BUCHANAN, A.M., Surgeon and Lecturer on Clinical Surgery, Glasgow Royal Infirmary.

In both of these diseases, but more especially in diphtheria, and more manifestly when they occur as epidemics, they may exist in one of two types, the sthenic or asthenic. In the asthenic, the tendency is to cause death by exhaustion—failure of the vital powers; in the sthenic, by suffocation. You will hear it sometimes stated that, because diphtheria is a general disease—somewhat like scarlet fever—and the lymphy effusion but the local manifestation of it, therefore it is unphilosophical to propose tracheotomy in such a case; but you will remember that this operation has never been suggested with any intention of cutting short the disease, nor ought it to be performed when there is great prostration. It is in the sthenic form alone that it is admissible. And what I would urge on you is this, when you find that your patient, whatever be the original disease—croup or diphtheria—is not improving by the treatment you have been adopting; when you find that the effusion is going on to produce suffocation; when the tendency is to death by apnœa more than by exhaustion, then you ought to step in and perform tracheotomy for the purpose of preventing immediate death, and so give longer time for the patient to live through the disease and ultimately throw it off. Nay, you will find, after a comparatively limited experience, that you will be able to recognise early in the progress of a case whether the ten-

dency is to apnœa or exhaustion; and I would be inclined to urge my own experience as a reason for performing tracheotomy in the class of cases in which it will inevitably be required, before the struggle for breath has exhausted the strength of the sufferer, so rendering the operation less successful than it might have been.

In short, let not the name of the disease deter you; but, when you find a patient clearly progressing to death by suffocation, while the vital powers are otherwise vigorous, my maxim for you would be, open the windpipe and ward off the impending death, whatever be the ultimate result: you have done your duty and saved life, at least temporarily. I admit that it is sometimes a very nice point to decide, when to interfere and when to refrain, and especially to discover when the effusion has spread down into the bronchi or bronchial tubes, in which case the operation would be useless. Percussion of the chest and the sibilant *râles* discoverable by auscultation are valuable aids to diagnosis; but there is one most characteristic sign which I find a sure and ready guide to distinguish between dyspnœa depending on pulmonary occlusion, whether congestive or bronchial, and that depending on tracheal obstruction—I allude to the observation of the respiratory movements. When the obstruction is in the larynx or trachea, the powerful attempts at respiration will be plainly visible, and their inefficacy will be evidenced by the drawing in of the costal cartilages and the intercostal spaces. When this is well marked and increasing, the pulse being moderately good, it is a proof of the vigour of the vital powers, and is a clear indication for tracheotomy.

The steps of the operation are very easily indicated, but very difficult in the performance. The great maxim is, "operate leisurely and without hurry." The patient having been put under the influence of chloroform—a very great assistance in this operation—an incision is to be made about an inch and a half long from half an inch below the cricoid cartilage downwards. Layer after layer of the cellular tissue is to be divided till the trachea comes into view. This, which is easily stated, is a troublesome matter, owing to the perpetual movements of the trachea and the bulging into the wounds of veins, cellular tissue, and in children the apices of the thymus gland, all of which must be held aside with retractors. Any vessel which bleeds must be tied; and it must be a principle that the second stage of the operation is not to be undertaken till the white rings of the trachea are clearly seen at the bottom of the wound. A sharp hook is now to be fixed in the upper part of the trachea brought into view, and the knife plunged into it—the back of the knife being towards the larynx—and the incision made half

an inch long. A pair of closed dressing forceps are now to be introduced into the slit and opened, on which there will be a violent struggle, then a forcible expiration expelling quantities of false membrane and viscid mucus; and presently the patient will lie quiet, breathing tranquilly through the opening. The silver tube is now to be introduced, and the operation is completed. The only special precaution which I adopt in the after-treatment is to order the air in the apartment to be kept moist by steam from a kettle, or by some other means.

The following case illustrates these points very well.

William S., aged 7 years, when convalescing from scarlet fever, had an attack of tonsillitis. The palate and tonsils became covered with a white exudation, and the voice became hoarse. After a few days he improved so much that there was no anxiety about him, when on the 12th January there was considerable difficulty in breathing and a hoarse barking cough. The white patch was to be seen on the tonsil, evidently extending downwards. On the 13th he continued in the same state, but on the 14th the symptoms had increased in severity so that Dr. Pirie, the medical attendant, requested me to visit him in consultation. When we saw him, however, the dyspnœa had become less urgent, so that we thought that he had a hope of recovery without operation. On the 15th all the symptoms became aggravated, and when I saw him in the evening with Dr. Pirie, it was quite evident that the struggle for breath would soon wear him out. The pulse was fair; he could swallow well; but the fits of dyspnœa were so frequent and urgent that he was tossing about, gasping, and begging to be relieved. An examination of the chest showed most clearly that the obstruction was in the larynx or trachea, and that the effusion had not extended into the smaller bronchi. The muscles were acting very powerfully, so that at each attempt at inspiration the sternum, costal cartilages, and intercostal spaces, were violently pulled inwards. The effort to inspire was evidently very strong, but there was hardly any room for air to pass; a clear indication for opening the windpipe to admit the air.

The nature of the operation having been explained to the parents, they committed the child to our care to do what we thought best for it. The assistance of my friend Dr. Smart having been procured, I proceeded to perform tracheotomy precisely as before described, and with the same result. The child, who before was tossing about in an agony of dyspnœa, was soon breathing with perfect tranquillity through the tracheal silver tube.

I need not describe the progress of the case from day to day. Four or five of my students most kindly volunteered to be with the patient for the first forty-eight hours, during which time

the child was never left without skilled attendance, Dr. Pirie visiting two or three times a day. The result was most satisfactory. The tube was removed on the eighth day, and after that the child made a rapid recovery.—*British Medical Journal. March* 4, 1871, *p.* 217.

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## 25.—CARBONATE OF AMMONIA IN THE TREATMENT OF PNEUMONIA.

By Dr. A. PATTON, of Vincennes, Indiana.

[Carbonate of ammonia has long been used in pneumonitis in its latter stages, on account of its stimulant properties; but it is probable that these constitute but a very small proportion of its remedial value. Dr. Patton has employed the remedy in ninety-six severe cases, not only at the latter stages of the disease, but from the very commencement. He finds that instead of increasing the febrile excitement and heat of surface, both were greatly reduced in a short time.]

It relieves, as we conceive, the hyperinosis and thus prevents many of the complications which are likely to occur during the progress of the disease, and that it diminishes the viscidity of the sputa, promotes expectoration, prevents embolism, and promotes the oxygenation of the blood.

If the remedy be given early in the disease, and regularly, in from five to ten grain doses, every two hours, night and day, it will, in almost every case, so limit the exudation process as not only to greatly lessen the amount of hepatization, but to insure a prompt and rapid absorption of the exudation, and occasionally it will entirely prevent hepatization, and terminate the inflammation by resolution. Upon these points I cannot be mistaken, as my observations have been extensive, and carefully made.

I have treated during the last five years ninety-six cases of pneumonitis upon the exclusive ammonia method, using no other medicine excepting that in the congestion stage I sometimes give sixty drops of chloroform to aid reaction. When an aperient is required I give sulph. magnesia. Blisters may be necessary in some cases. No alcoholic agents are allowed in any stage of the disease.

Quinia is sometimes given as a tonic in the advanced stage, and may be required to control malarial influences, but is not advisable in the early stage.

In the three years from 1862, when I first began to employ the remedy, until 1865, I pursued a mixed treatment, sometimes employing alcohol, alternating with ammonia, and some cases were treated with veratrum viride, while others received

tart. ant. and calomel. It was during this transition period that I was enabled, by comparative results, and effects of remedies, to determine absolutely in favour of ammonia.—*Amer. Jour. of Med. Science, Oct.*, 1870, *p.* 374.

## DISEASES OF THE ORGANS OF DIGESTION.

### 26.—ON AN OUTBREAK OF DIPHTHERIA IN THE OBSTETRIC WARDS OF GUY'S HOSPITAL.

By Dr. J. Braxton Hicks, F.R.S., Physician-Accoucheur to Guy's Hospital.

[In December 1869, an attack of diphtheria began first in a patient recently admitted with anasarca coupled with pregnancy. The disease extended to others, but it was remarkable that those only were seriously attacked who had had some surgical interference—these were sixteen in number.]

What connection there may be between scarlet-fever poison and diphtheria it is impossible to say. It is a curious fact, however, well known to many engaged in general practice, that after some cases of diphtheria have occurred in a house among children it may cease, and scarlatina then make its appearance. In a school, of which I have knowledge, of about forty boys three were taken ill of diphtheria, whereupon that disease ceased to appear, but was replaced by scarlatina. Numerous instances have occurred in this way. This does not prove their being of the same kind, because we know measles to occur in collections of children shortly after an attack of scarlatina; still, it is not common (as far as I am aware) that in a school the first boys attacked should have measles, and those later on scarlatina.

Be this as it may, up to the period of the case of scarlatina in our wards there was no illness like that which succeeded.

Of course it may be argued that all the cases reported were not affected by the diphtheritic influence, and this objection cannot be positively answered; but such results as are here mentioned had been very rare up to that period, nor have they since recurred. And inasmuch as all the cases occurred within a fortnight of one another, and that the first and last case had certainly diphtheria, it is a fair inference that an untoward influence derived from that source had marred that recovery which we had every reason, from ordinary experience, to expect.

It is the habit of looking so much for the ordinary symptoms of a specific poison which hinders us from recognising the untoward influence the same poison may exert in a system exposed

to any lesion which requires good health for reparation. So small a quantity of baneful influence is sufficient to produce imperfect reparation of damaged tissues, and so slight is the line of separation between the tendency to good or bad recovery, that we are apt, till our attention is closely drawn to the subject, to overlook these important influences. That which one often puts down to a non-disposition to "heal" is quite as often, if not more often, dependent on the system being influenced in a less degree by a poison which in greater quantity would produce the violent forms of pyæmia. Let this be fully recognised, and careful attention will be amply rewarded by far more satisfactory results. Numberless causes of a detrimental kind surround us all, and the utmost care is required lest they affect those who require reparation of damaged parts. This is equally true in the surgical as in the lying-in room. It matters not in practice whether we believe in the living germ theory or in floating poison; but that what attacks the patient has a material existence, and is capable of being diffused, driven away, or destroyed, seems to be completely proved.

That it can enter the system either by the lungs or by the abraded surface is also clear, and that it acts more rapidly by the latter means is more than possible.

However, I have thought the narration of these cases may assist in our knowledge of the matter, and the reader is at liberty to draw his own conclusions from them.

*Case* 1.—A patient was admitted five months pregnant, having general anasarca, with albuminuria. After being in a few weeks she spontaneously aborted. The placenta came away well. After about five days she was feverish, and had pain in the lower part of the abdomen. On examination I found the fold between right thigh and swollen labium covered by a large diphtheritic patch four inches long by two broad. Although this became less in extent, she every day became lower, the tenderness in the lower part of the abdomen greater, and a decided hardness felt to be within. She vomited, and had furred tongue, rapid pulse, and at the end of ten days after confinement died. The post-mortem examination showed kidney disease (Bright's) rather recent; peritonitis extensive, with effusion of lymph in left groin enclosing purulent fluid.

*Case* 2.—While the first patient was succumbing I removed a fragile calculus from the urethra of a woman who had just entered the hospital. She went on well for about three days, when she complained of being feverish and tender in the urethra. On examination the parts, which had been abraded, were covered by diphtheritic layer running up to the bladder. In a few days cystitis came on. To relieve her pain morphia was injected into the arm. The spot inflamed and suppurated.

About three days after this a swelling over one knee came on. Matter in a day or two was found and let out. On her shoulder also the same occurred. In the mean time her constitutional symptoms became very severe, and it was evident that pyæmia was present in its worse form. As she was sinking, and her husband was anxious for her removal home, she was taken out, and died shortly after.

*Case* 3.—At the same time this last narrated case was operated on another patient underwent a partial operation for atresia vaginæ, extending about two and a half inches, the intention being to complete it in a few days. She was, however, taken with severe feverishness, great local tenderness, and cellulitis on one side of the uterus. She remained very feverish for one month, the pulse being 110 to 120, P.M., and the temperature 100° to 102° constantly. Of course it is well known that vaginal operations are occasionally attended by diffuse inflammation, and even fatal peritonitis. Whether this were so in this instance I cannot say, but I had not had any such result before.

This patient ultimately recovered.

*Case* 4.—Within a few days of these cases a woman was tapped for compound ovarian dropsy. The trocar went through solid portion, and though introduced twice it failed to draw off fluid. The cyst was very high up and posterior, and could not be tapped from the front even three inches above the umbilicus. She had symptoms of inflammation of the cyst throughout, and the wounds caused by the tapping became very inflamed, and a blister spread from them for an inch and a half. Decomposition of the fluid in the cyst took place, fœtid gas being largely generated, which was drawn off by the exploring trocar twice, at so high a point towards the epigastrium that it would have been dangerous to have used a large trocar in case the intestine had been in front. She lived after the tapping about three weeks. It was found that the whole tumour, which was very compound, had become inflamed, putrid contents being in the cysts.

*Case* 5.—At the same time another woman, with compound ovarian tumour, was tapped in the largest cyst. Suppuration of the lining membrane walls of that cyst took place, attended with constitutional symptoms of such severity that her death appeared imminent. The vomiting was incessant, and injections per rectum were employed for some days. She somewhat improved, but the cyst rapidly filled, and, when it was about a month after tapped, a large quantity of purulent fluid was drawn off. The cyst was washed out with weak iodine water, and the irritative symptoms subsided. When she was some time after again tapped pus was absent. This patient recovered.

Whether the irritation of the cyst in this case was owing to the atmospherical condition of the ward no one can pronounce, but taken in combination with others it seems to me to point to this conclusion.

*Case* 6.—At the same time an elderly woman was under treatment with malignant disease of the bladder. She was attacked with severe sore throat and slight diphtheroid exudation, coupled with high feverishness. From this she ultimately recovered, though she subsequently died of the malignant disease. It was after dilatation of the urethra, and removal of a large bulging mass from the wall of the bladder, that the symptoms appeared.

*Case* 7.—Towards the termination of these cases a woman about seventy years old was admitted, suffering very severely from intense tightness of the abdomen caused by a semi-fluctuating ovarian tumour. It was considered necessary to tap her; but the contents were gelatinous lumps, to be abstracted only by much assistance from pressure and clearing of the canula. About three fourths of the contents of the principal cyst was obtained. Next day she went on tolerably well, but the opening discharged fluid. On the third day this became offensive and plentiful, producing a fœtor in the ward.

The pulse was about 110, and the temperature 97°. Finding these symptoms not in accordance with the state of the cyst, and knowing that they were like the effects of diphtheria, I examined the throat, in which she insisted she had no distress. However, the whole fauces were covered with a thick diphtheritic layer.

She continued in this state, with cold skin and slow pulse, for three days, when she died, the smell from the cyst-discharge being almost unbearable, although the cyst was twice a day washed out with disinfectants.

*Case* 8.—About the middle of the fortnight during which this attack lasted a young woman with inversion of the uterus was placed under chloroform, and an attempt at restoration was made. It was unsuccessful, but next day symptoms of much feverishness occurred, and subsequently tenderness of uterus. Shortly after anasarca came on, with albuminuria coupled with long and severe vomiting. She ultimately died of uræmic poisoning. Recent Bright's kidney was found, and an abscess in the ovary running up the psoas nearly to the left kidney. Now, she had been subjected to the same treatment before without irritation, and it is curious that the symptoms were general at first. The kidney complication is also very remarkable.

After the death of this patient the ward was emptied of the remaining patients, and thoroughly disinfected.

It is proper also to observe that about the commencement of this attack there were some cases of malignant uterus in the same division of the ward, the discharges from which were very offensive. As the result of experience we know it is not well to keep many cases of this kind in one ward, for whenever the effluvium is powerful it has a distinctly detrimental effect on other patients.—*Guy's Hospital Reports*, 1870-71, *p.* 165.

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## 27.—THE ACTION OF MERCURY ON THE LIVER.

That mercury does not stimulate the liver, or increase the flow of bile in any way in health, but tends rather to bring about an opposite result, was the conclusion drawn from a series of experiments on dogs, conducted by a special committee appointed by the British Medical Association. Dr. Hughes Bennett has followed up the investigation which resulted in establishing the above conclusion by the performance of another set of experiments, having for their object the determination of the question whether—as affirmatively asserted—"mercurials possess any specific power of exciting the biliary secretion, by acting on the orifice of the common bile-duct, and so stimulating the secretion, through the nerves which connect it with the liver, just as pyrethrum or vinegar stimulates the salivary glands when it is applied to the orifices of the salivary ducts." These experiments have led Dr. Bennett to conclude that the drug has no such power. Several mercurial compounds used as medicines were treated in such a way as to approximate their condition to that which they would assume when digested for a while, and then applied to the orifice of the common bile-duct. For instance, blue pill rubbed up with milk was given to a cat, which was killed in an hour afterwards; the contents of the stomach being kept for three hours at a temperature of 100° F. Some of this fluid, portions of digested mixtures of blue pill and milk and rennet, calomel and milk, and corrosive sublimate and milk, were applied to the orifice of the common bile-duct of a chloroformed rabbit at different times, without exciting the escape of bile. The same result followed the application of a mechanical irritant, acetic acid, powdered calomel, bichloride of mercury, and the two poles of an interrupted electric current, to the orifice of the duct, or of the electric current to the gall-bladder itself, or the liver. But one occurrence of practical significance was noted. When the abdominal or thoracic muscles were excited by the induced current, then a free flow of bile from the common duct occurred. Dr. Bennett denies the presence of any muscular coat in the gall-bladder, and the possibility of its being excited to contraction by reflex irritation; and concludes that mercurials are not cholagogues

in any sense of the word, neither are substances that irritate the orifice of the bile-duct, but that pressure exerted upon the liver and gall-bladder by the contractions of the surrounding muscles cause the bile to flow into the duodenum; hence the value of exercise in certain bilious complaints.—*Lancet, Jan.* 14, 1871, *p.* 58.

## DISEASES OF THE URINARY ORGANS.

### 28.—ON CERTAIN MORBID CHANGES IN THE NERVOUS SYSTEM ASSOCIATED WITH DIABETES.

By Dr. W. HOWSHIP DICKINSON, Physician to the Hospital for Sick Children; Assistant Physician to St. George's Hospital.

[The view that a functional disorder is to be attributed to erroneous action rather than to structural change must be looked upon as not strictly correct. Probably all temporary disturbance of function has its source in a transient alteration in the solids or fluids of the body. The discovery of Bernard that puncture of a certain part of the medulla oblongata rendered the urine saccharine, rendered it probable that some undiscoverable alteration in structure exists in this part of the nervous system in cases of diabetes. The method of Dr. Lockhart Clarke has enabled the writer of this paper to discover that the nervous centres are really the seat of important alterations of structure. He has carefully examined five cases.]

The results obtained in the five cases examined are briefly these; peculiar morbid changes were constantly found in the cerebro-spinal system. In all the alterations were of the same nature, and in similar situations. The earliest alteration recognised consisted in a dilatation of the blood-vessels, particularly of the arteries, with accumulation and frequent extravasation of their contents. The next was a degeneration of the nervous matter at certain points outside the swollen vessels, probably caused by the intrusion into it of blood-corpuscles. The degenerative process occasioned destruction and excavation of the tissue round the vessel. Cavities were thus produced, often large enough to be striking objects even without the microscope, which contained blood-vessels, extravasated blood, grains of pigment, and the products of nervous decay. Finally, the contents appeared to become absorbed, so that simple vacuities were left. The perivascular sheath was variously stretched and altered in character and became loaded with pigment, but it seemed that these alterations were consequent upon the dilatation of the vessel, extravasation of blood, and excavation of nervous matter.

As to their situations, the changes occurred in constant association with arteries. They were found in every part of the spinal cord and encephalon, attaining their greatest development in the medulla oblongata and pons Varolii. The excavations were generally the most marked where the blood-vessels piercing the brain were the largest and most numerous. They were frequently in connection with folds of the pia mater. The regions affected with the greatest frequency were the olivary bodies, the vicinity of the median plane of the medulla, the grey matter of the floor of the fourth ventricle, a fissure just internal to the origin of the facial nerve which lodges a process of pia mater, and a depression similarly occupied which penetrates from between the anterior crura towards the centre of the pons Varolii.

The optic thalami and corpora striata were involved, though to a comparatively slight extent. The septum of the ventricles and the white matter of the convolutions displayed the alterations in a remarkable manner. The changes especially affected the white matter, though the grey matter at the floor of the fourth ventricle and of the spinal cord are exceptions to this statement.

In the cord the most conspicuous change was the enlargement of the central canal probably connected with degeneration of tissue, of which many evidences were found there and elsewhere.

The nerve-cells of the brain and cord were generally perfect.

Such parts of the sympathetic system as were examined, namely, the upper cervical and semilunar ganglia, were apparently natural.

The only constant change found in the viscera was epithelial accumulation in the liver and kidneys.

So far as the foregoing observations extend it would seem that diabetes is associated with a substantial change, which follows the arteries of the brain and cord, and comprises injection, extravasation, and destruction of tissue. From the uniformity with which such alterations were found in the five cases examined, it may be presumed that there was a more than accidental connection between the symptoms of diabetes and the peculiar state of the nervous centres. The question at once presents itself whether the lesion is a result of the change of secretion or is antecedent to it; or have the lesion and the symptom no closer connection than as the effects of a common cause?

The last hypothesis may be dismissed as unsupported by our present knowledge. If such a common cause exists it has hitherto eluded our observation.

The association of the morbid change with the blood-vessels

suggests that it may be a result of the state of the blood. We know that the blood in diabetes is altered by the presence of sugar; Is it to this that the deterioration is due? Several considerations militate against this, at first sight, probable explanation. The veins and capillaries appeared to take no share in the morbid process, though they must be equally permeated by diabetic blood, which, in the capillaries at least, is brought into more intimate relation with the tissues than in the arteries. If the changes in the tissue were directly due to the altered composition of the blood the capillaries could hardly fail to distribute its morbid influence. Beside this, the blood traverses the whole body without producing in any other organ an analogous failure of nutrition. We may probably abandon the view that the changes described are consequent upon the diabetic state of blood, and have recourse to the supposition that the nervous alterations are antecedent to, and productive of, the glycosuria.

The following considerations give likelihood to the belief that the nervous changes are primary. No organic alteration has been found elsewhere to which the saccharine state of the urine can be attributed. The changes in the brain are in their nature and situation such as physiology has shown to be capable of producing that symptom. Further, we are the more disposed to regard the condition of the nervous centres as primary by the fact that alterations similar in kind, though differing in distribution, occur as belonging exclusively to the nervous system, quite independently of diabetes. This is the case particularly in the general paralysis of the insane. Dr. Lockhart Clarke has, since most of the preceding descriptions were written, described the lesions observed in that disorder, and shown that, though differing somewhat in situation, they are of the same nature as those here described. In general paralysis the change chiefly effects the convolutions and pons varolii, leaving the lower part of the medulla and cord natural; there is also wasting of the nerve-cells. In diabetes, as has been seen, the medulla is affected throughout, the nerve-cells remaining perfectly natural.

The pathology of the nervous system is yet in its infancy. The minute anatomy of many disorders which are well known clinically has still to be unfolded. We cannot tell how far such alterations as are associated with diabetes and general paralysis are common to other cerebro-spinal disturbances. A loss of arterial tension in nerve-tissue appears to be the first recognisable step in tetanus as in diabetes, and it is not improbable that some such morbid process as has been described in this paper may prove to be a mode of nervous failure producing different symptoms, and known by different names according to its location in one part or another of the nervous structures.

The conclusion that diabetes is primarily and essentially a nervous disease accords with all that is known of its natural history; indeed, the opinion has of late years been gaining ground that the disorder is due to altered nervous action, though no structural change was known to account for it. The urine often becomes saccharine in consequence of injuries of the head, apoplectic seizures, intra-cranial tumours, and other sources of cerebral irritation. These traumatic and accidental forms of the disease are, as we may believe, not necessarily accompanied by the grave and general lesions which have been under consideration. Several kinds of irritation, if acting in the right situation, give rise to a similar change in the urine, though often temporary, and for the most part, though not always, unaccompanied by the serious symptoms which characterise what may be termed "idiopathic" diabetes.

Diabetes in its ordinary "idiopathic" form, though sometimes hereditary, and often taking its origin in causes which are not within our knowledge, continually results from circumstances which exert a depressing or otherwise injurious action upon the nervous functions; among them may be mentioned mental disturbances, rage, grief, anxiety, and intellectual toil, and the various forms of dissipation, among which sexual excess takes a prominent place.

Causes of this nature may readily give rise to modifications of circulation in the nervous centres, and it has been shown that, as far as could be learned from the microscope, a widening or distension of the arteries is the initial change in the pathological series.

The foregoing observations concur in leading to the belief that diabetes essentially belongs to the nervous system, a consideration which must have a practical issue in modifying the treatment of the disease.—*Med.-Chir. Trans.*, 1870, *p.* 251.

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## 29.—ON THE TREATMENT OF NOCTURNAL ENURESIS AND AN ALLIED AFFECTION.

By Dr. J. B. BRADBURY, B.A., Physician to Addenbrooke's Hospital, Cambridge.

In the *Lancet* for Nov. 19, 1870, Dr. William Thompson, of Peterborough, has recorded two cases of nocturnal incontinence of urine treated successfully by hydrate of chloral. Dr. Thomson's first case was that of a girl, aged 12 years, who enjoyed perfect health, with the exception of wetting her bed every night, and of having a frequent desire to micturate in the daytime. After taking one dose of fifteen grains of hydrate of chloral, the nocturnal symptoms were checked, and, after a fortnight's treatment by the same drug, the day symptoms also

disappeared. The second case was that of a boy, aged 13 years, who for seven years had had nocturnal enuresis. This boy was also cured of this distressing malady after taking one dose of the medicine.

A short time after reading Dr. Thomson's note, I had an opportunity of putting this drug to the test in this affection. My patient was a girl, aged 15 years, who had wetted her bed every night for nine years. She was ordered fifteen grains of hydrate of chloral every night, and after taking the first dose of the medicine there was no return of the complaint. At the end of six weeks, she had had no relapse of her infirmity.

Being satisfied with the efficacy of this drug in nocturnal enuresis, I next resolved to try its value in a closely allied functional disorder, viz., nocturnal incontinence of semen. In many cases, both of nocturnal incontinence of urine and of semen, there is reason to believe that spasm is an important factor. As you are all aware, the bladder is furnished with two sets of muscular fibres—the detrusor urinæ and the sphincter vesicæ—the latter being the antagonist of the former. In a healthy person, these two sets of muscular fibres counterbalance each other; but under the influence of volition, as when we wish to micturate, the detrusor muscle can overpower the sphincter, which can be controlled by the will. In cases of nocturnal enuresis, the influence of the will on the fibres of the sphincter can, when the person is awake, enable this muscle successfully to oppose the spasmodic contraction of the detrusor urinæ, and thus prevent diurnal enuresis; but when the patient has gone to bed and fallen asleep, this controlling power of the will is lost, and thus the sphincter is unable to counteract the action of the detrusor, and as a consequence of this, the urine escapes from the bladder. In many cases of spermatorrhœa, also, Trousseau was of opinion that the vesiculæ seminales entered into a state of erection, like the detrusor vesicæ muscle, and that semen was ejected in consequence of this excessive contractility. It is, also, by no means uncommon to find that persons, who are troubled with nocturnal incontinence of urine in boyhood, suffer from nocturnal incontinence of semen when they have arrived at the age of puberty; and occasionally the two affections may coexist in the same person, as in a case which came recently under my notice.

A gentleman, aged 27, consulted me on Feb. 1, 1871, for nocturnal emissions and incontinence of urine. He had been troubled with the former for nine years, but had never wetted his bed before the previous night. He found the spermatorrhœa worse when in Cambridge, scarcely ever having passed a night without one or more emissions. He informed me that a brother who died had enuresis when a boy. The patient passed urine

in a good stream; his bowels were regular; he had at times severe attacks of neuralgia. His health was completely undermined by the spermatorrhœa; he felt exceedingly feeble, and unable to exert himself either mentally or bodily. He had taken strychnia and other remedies without benefit. I ordered him fifteen grains of hydrate of chloral every night, and recommended him not to drink spirits at night, which he informed me he seldom did. On February 4th, there had been no return of the spermatorrhœa or of the inability to hold his urine during sleep. On February 22nd, my patient said he felt quite well, and had had no return of his complaint, which gratified him very much, as he was shortly to be married.

My next case was somewhat similar to the above, except that there was no difficulty in retaining the urine. The nocturnal emissions also occurred less frequently, and the patient had only been troubled with them for nine or ten months. The result of the treatment by chloral hydrate was quite as satisfactory as in the preceding case; and, as a proof that the medicine was really efficacious, I may state that the patient one evening forgot to take his medicine, and the consequence was that his infirmity returned.

Whenever, therefore, there is reason to believe that nocturnal urinary and seminal incontinence are due to spasm (not the result of irritation reflected from a fissured anus, worms, urinary calculi, congenital phimosis, or of structural disease of the walls of the bladder, &c.), hydrate of chloral will be found a most serviceable drug in their treatment, in consequence of the acknowledged power of this drug of allaying spasm, as observed in tetanus and other spasmodic disorders.

If I were asked what advantages I claim for hydrate of chloral over belladonna in the treatment of these affections, my answer would be: 1. That the effect of belladonna is not so immediate, frequently taking weeks to produce any marked control over the disease; whereas the influence of chloral hydrate is most rapid, the malady frequently disappearing after the first dose of the remedy. This quick improvement cannot be overestimated in the treatment of these affections, upon which the mind exerts a powerful influence. 2. That belladonna, sometimes induces profuse diarrhœa, a result which I have never known chloral hydrate to produce. 3. That belladonna, when pushed to the extent to which it is necessary to be really efficacious, not infrequently impairs vision, &c., which is not the case with chloral hydrate. 4. That belladonna has, in my hands, on several occasions failed to be of any service.

Of course I am aware of the fallacy of the reasoning *post ergo propter;* but, on theoretical grounds alone, I am of opinion that a more extended trial of the chloral hydrate in these affections will establish the therapeutic value which is claimed for it.

There is one other point in connection with nocturnal enuresis which has interested me very much, and that is the close similarity between this affection and epilepsy; indeed, nocturnal enuresis might, without any great error, be called epilepsy of the bladder. I will, therefore, conclude my remarks by stating the points in which I think the analogy holds.

1. Enuresis and epilepsy are both markedly hereditary, and one neurosis may be transformed into the other; patients who have had incontinence of urine in youth sometimes becoming epileptic after puberty.

2. Both affections are influenced by the same system of nerves —the sympathetic, which may, under certain circumstances, induce spasm in the muscular fibres of the small arteries of the brain, as it does in the unstriped muscular fibres of the detrusor vesicæ muscle.

3. Belladonna is of service in the treatment of both these affections, and probably acts by its influence on the sympathetic.

4. Epilepsy may be either essential or due to reflex irritation, and so may nocturnal enuresis.

I have not tried hydrate of chloral as a remedy in epilepsy; but I am of opinion that it will be found useful in the treatment of some forms of this affection.—*British Medical Journal, April* 8, 1871, *p.* 363.

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## 30.—INCONTINENCE OF URINE, AND ITS TREATMENT BY A NEW REMEDY.

### By Dr. JOHN BARCLAY, late Assistant-Professor of Materia Medica and Medical Jurisprudence in the University of Aberdeen.

During the past two years and a half, twenty cases of incontinence of urine have been treated by me; the medicine invariably prescribed has been syrup of the iodide of iron alone, and so far as I know there has been no failure. I have notes of all the cases, but only eleven in a completed state, since the other nine, who came from a distance, did not return to say what was the result. The probability is that they were cured, otherwise they would not have been got rid of so easily. Uncured cases are those that return upon our hands. At all events, the eleven who did report themselves, or who were continually under observation, were all cured, the improvement in several of the cases following so closely on the administration of the remedy as to leave no doubt but that the good effect was due to the syrup. I may mention that Dr. Manson, of Banff, and Dr. Smith, of Kinnairdy, have both found the medicine equally satisfactory. Dr. Smith says that he tried it only a fortnight ago, in a boy who for a long time had been a sad martyr to

both diurnal and nocturnal incontinence, and who had resisted all other remedies, but upon giving him the iodide, in two or three days he was all but well.

I now give shortly the eleven cases of which I have completed notes, and the first of these is that which suggested to me the remedy.

*Case* 1.—April 13, 1868. Helen W., aged 14 years, has impetigo of the head and face; ordered half-drachm doses of syrup of the iodide of iron three times a day, and some diluted citrine ointment as a local application. April 30. Reported cured. From this time down to June 12 she got no medicine, when the girl herself came to me, telling me she had nocturnal incontinence. In the hurry of the moment, and without asking any questions or her volunteering any statement about the duration of the complaint, I ordered tincture of iron. She continued to take this till October 9 without any benefit, when I ordered tincture of belladonna. She returned on December 2, saying this, too, had done her no good. Her mother, who accompanied her, now told me that during the time the girl took the medicine for the eruption on the head and face, and for about a month after, she had no incontinence, and that the complaint, which had existed from childhood, had defied every means tried to cure it up to that time. It had, however, returned, and she wished to get the same medicine. I ordered it as before, and on December 23 she returned to say she had wetted the bed only four times since she got the medicine. I repeated it. On February 6 she reported that she had only had incontinence twice since last date, and none at all for the last twenty days. April 1. Has not wetted the bed since last date, and only twice since December 23. I have often seen this girl since, and she has had no return up to the date of my writing.

*Case* 2.—Dec. 5, 1868. James S., aged 10 years, a poor scrofulous-looking creature, with cough and purulent sputa, and other phthisical signs and symptoms, no appetite; ordered iodine externally, and syrup of the iodide of iron in twenty-five-minim doses after meals (I heard nothing at this time of the incontinence). December 22. Decidedly improved; cough better, and he eats better. I was told to-day that he had laboured under incontinence of urine at night for some eight years, without even passing a night, but that since he had got the mixture he had only wetted the bed three times. To increase the syrup to half-drachm doses, and to take cod-liver oil. This boy was in a few days more cured of the incontinence, but in April, 1869, he died of phthisis.

[Nine other cases are related.]—*Medical Times and Gazette, Dec.* 17, 1870, *p.* 697.

## 31.—ON THE DIAGNOSIS AND PROGNOSIS IN CASES OF BRIGHT'S DISEASE.

By Dr. GEORGE JOHNSON, Professor of Medicine in King's College, and Physician to the Hospital.

Chronic Bright's disease, with the anatomical character of a large white smooth kidney, like the small red kidney, is sometimes the result of an insidious chronic degeneration; but, unlike it, the large white kidney is often a sequel of an acute inflammatory attack ("acute disquamative nephritis"), and we are thus enabled in some cases to determine the exact period at which the disease began. We may learn that, some months or even years ago, the patient had acute Bright's disease and dropsy; that the dropsy disappeared, but the urine never ceased to be albuminous. The history of these chronic cases sometimes extends over a period of many years. One case that came under my observation terminated fatally after albuminuria had continued for more than twenty years, one at the end of eleven years, and one after an illness of ten years. These were all cases of enlarged white kidney. Some writers have stated that the cases of chronic Bright's disease which have the slowest and the longest course are those associated with the small red granular kidney; but this statement does not accord with my experience. I have met with no cases of small granular kidney in which the disease has been known to exist for so long a period as in the cases of the large white kidney to which I have just now referred. It must, however, be borne in mind that the course of the disease which results in the small granular kidney is often so insidious, that it may have existed long before it was recognised or suspected.

In some cases, we may trace back the probable commencement of a chronic renal degeneration to a period when the patient began to be troubled to rise once or oftener in the course of the night to pass urine. This increased frequency of micturition may be a result either of a more abundant secretion of urine, or of some abnormal and irritating quality of the secretion acting as an irritant upon the bladder. With a history pointing to the existence of a large white kidney, the condition of the urine may vary considerably. The most favourable appearances are, a natural sherry colour, with normal specific gravity, a moderate amount of albumen, absence of all sediment and tube-casts, or a light cloudy deposit containing small hyaline casts; the amount of urine secreted being about normal. As the structural changes in the kidney increase, the urine loses its natural sherry colour, and at length it may become almost as colourless as water. The measure of urine secreted varies considerably, being sometimes much in excess of the normal amount, more frequently below it. The dropsical symptoms, as

a rule, bear an inverse relation to the amount of urine secreted, and a direct relation to the drain of albumen from the blood. The dropsical tendency, too, is favoured by the dry and inactive state of the skin which is commonly associated with chronic renal disease.

The gland-cells in the advanced stages may undergo a partial oily transformation, or they may be replaced by unorganised fibrine, which fills the tubes. Both these changes are indicated by the character of the tube-casts: the oily transformation by oily casts and cells; the fibrinous exudation by large hyaline casts having a diameter equal to that of the tubes in which they are moulded. When urine of pale colour and highly albuminous, deposits a copious and rather dense whitish sediment, mainly composed of large hyaline and large granular casts, we may be sure that the glandular tissues are becoming rapidly disorganised, and that uræmic symptoms will speedily occur. I know of no appearances in the urine which are of more unfavourable import than those which I have just now described. the microscopic appearances to which I refer sometimes present themselves in the more advanced stage of a case in which, at an earlier period, oily casts and cells have formed the chief microscopic sediment. This sequence of events occurred in a case which I watched from its commencement to its termination, during a period of ten years. The case was one in which a large white granular fat kidney subsequently underwent a process of atrophy and contraction, the progress of the disease in its different stages being clearly indicated by the microscopic appearances in the urine. The preserved specimens from which these drawings were taken I have in my possession, and they still retain their characteristic appearances. The disease began as an attack of acute disquamative nephritis. This passed into a chronic form of disease, with the appearance of oily casts and cells. After a period of nine years, these were associated with, and afterwards replaced by, large granular and large hyaline or waxy casts. The kidneys were much reduced in size, their combined weight being only seven ounces and three-quarters; but their appearance was very different from that of a small red granular kidney. Their cortical substance was pale, and presented numerous yellow oily granulations, showing clearly that in this case contraction of the kidney had followed upon enlargement, with fatty degeneration of the glandular tissues.

I have before stated that the urine in cases of acute Bright's disease is usually more or less tinged by admixture with blood, which probably escapes from the ruptured Malpighian capillaries. During the progress of the various forms of chronic disease, the walls of the minute arteries become hypertrophied, and those of the Malpighian capillaries gradually become

thickened. Consequently, the capillaries are less liable to rupture, and the urine is rarely blood-tinged. When the urine is highly albuminous, of low specific gravity, and not only free from blood-tinge, but pale, from a deficiency of its normal colouring matters, there is reason to suspect the existence of advanced chronic renal disease. On the other hand, the appearance of dark coloured, smoky, or blood-tinged urine is, *pro tanto*, evidence of a recent acute affection of the kidney. I must warn you, however, to be on your guard against an occasional source of fallacy. It is this: in the advanced stage of chronic Bright's disease, the blood becomes much deteriorated, partly by the loss of its normal constituents, and partly by the retention of urinary excreta. There is, consequently, a tendency to hemorrhage from various mucous surfaces—from the nose, the lungs, the stomach and intestines, from the uterus, and sometimes from the bladder. The hemorrhage from the bladder gives the urine the blood-tinged appearance which it often has in cases of acute Bright's disease, when blood escapes from the substance of the kidney. You may come to a right judgment in these cases by a careful consideration of the past history, together with a close inspection of the urine. You will probably find that there are no blood-casts of the tubes, as there are when the substance of the kidney is the source of the bleeding. You may find some of those forms of tube-cast which point to the existence of chronic rather than recent acute disease: for instance, oily casts, or large granular and large hyaline casts. You may also find that the urine, when, after standing for a time, it has deposited the blood, presents the pale colour which is indicative of chronic disease in an advanced stage. When a doubt exists as to the renal disease being of recent origin or of long standing, the evidence of hypertrophy of the left ventricle of the heart, without valvular disease, but with a full and firm radial pulse, the result of excessive resistance to the passage of the blood through the minute systemic arteries, whose muscular walls are also hypertrophied—this evidence points to the existence of chronic renal disease. (Upon this point, see a paper by the author, *Brit. Med. Journal*, April 16, 1870.)

In cases of the large white kidney, the mode of death is usually different from that which is of most frequent occurrence in cases of the small red granular kidney. Uræmic convulsions, coma, and typhoid symptoms, are less frequent; while, on the other hand, an excessive dropsical accumulation, causing great distension of the skin, often results in fatal inflammation and sloughing of the integuments. In other cases, a dropsical accumulation within the chest, or engorgement and œdema of the lungs, may cause death by apnœa. In all forms of chronic

Bright's disease, gastro-intestinal symptoms, vomiting, and purging, the result of the vicarious elimination of urinary excreta, may induce a fatal exhaustion.

Some recent writers affirm that impairment of vision, the result of degeneration of the retina, with or without hemorrhage into its tissue, is almost exclusively associated with the small granular kidney. It so happens that most of the cases of retinal degeneration that I have seen have been associated with the large white kidney.

In the advanced stages of valvular disease or degeneration of the muscular walls of the heart, and in cases of emphysema of the lungs, with bronchitis and consequent retrograde engorgement of the systemic venous system, passive congestion of the kidney frequently causes not only dropsy, but also albuminuria and even hæmaturia; the urine containing small hyaline and blood casts. The secondary character of the renal complication is usually apparent from the history of these cases; and the diagnosis may sometimes be confirmed by the fact that, when the circulation has been relieved by rest in bed, by hydragogues and other suitable remedies, while the physical signs of the cardiac disease remain, the albumen disappears from the urine, to return, perhaps, when the circulation again becomes more embarrassed within the chest. I have seen this happen again and again during the progress of the same case.

When, in consequence of an obstruction at the heart, the systemic veins become overfull, the distension of the renal vein, acting backwards, through the intertubular capillaries, causes engorgement of the Malpighian capillaries, and a consequent transudation of serum through their walls. This serous transudation, mingling with the urine, renders it albuminous. Sometimes the distended capillaries have their walls ruptured, and blood escapes into the tubes and tinges the urine. It is interesting to note the action and reaction of renal and cardiac disease upon each other. In one case I observed the following sequence of events. A gentleman had acute Bright's disease with anasarca, from which, after many months, he completely recovered. During the progress of the disease there was developed a systolic bellows-sound over the base of the heart—the result, probably, of uræmic endocarditis. The bellows-sound remained after all symptoms of renal disease had passed away; and in time it became a double basic murmur, with the usual physical signs of incompetent aortic valves. For some years the valvular disease caused but little functional disturbance; but at length, with increasing obstruction of the circulation, dropsy came on, and with it albuminuria from passive congestion of the kidney. Death occurred in about ten years from the commencement of the renal disease. In this case it

will be seen that a primary renal disease was the exciting cause of valvular disease of the heart; and that, the renal disease having passed away, the cardiac disease ultimately caused dropsy and a secondary albuminuria, the result of obstructed circulation and passive congestion of the kidney.—*Brit. Med. Journal, Jan.* 14, 1871, *p.* 27.

## 32.—PERCHLORIDE OF IRON IN PARALYSIS OF THE BLADDER.

By C. H. WATTS PARKINSON, Esq.

On January 24, 1870, I was sent for to attend W. B., aged 74, a master blacksmith. He has for the last few years been subject to attacks of retention of urine, which have, however, hitherto been relieved by warm baths, &c., without the use of the catheter.

On my arrival, I found him suffering greatly from retention of urine, which had not been relieved by the means before used. I found the prostate enlarged, but passed a full-sized prostatic catheter, and drew off a large quantity of urine without much difficulty.

The next morning I was again obliged to pass the catheter; and the bowels being confined, I ordered a simple aperient.

Jan. 30. The catheter has been passed twice daily. There is stillicidium urinæ, but no power of passing water voluntarily, and the urethra is becoming irritable. Bowels very constipated, requiring frequent aperients.

Feb. 5. The old man is getting in a low, typhoid state. Tongue dry and brown. Pulse very feeble. A considerable amount of hemorrhage follows each introduction of the instrument. Urine loaded with phosphates and lithates, with mucus, epithelium, and some decomposed blood. Not the slightest attempt at contraction of the bladder.

6th. I drew off the urine, as usual, morning and evening, and then injected six ounces of a weak solution of tincture of iron, which I retained in the bladder for about half a minute.

7th (evening). There is a slight contraction of the bladder after the injection this evening.

8th (evening). The bladder contracts well on the instrument after the injection, and he has passed small quantities of urine occasionally during the day voluntarily, and there has been no overflow.

9th (morning). Has made water during the night. Bladder moderately empty. Urine getting better.

From this time he continued to improve, until February 18, when he could pass water better than he had done for some time.

Dec. 12. Has had no attack of retention since, and is able

to pass water freely, and is better than he has been for some years, and able to follow his usual occupation.

There could be no doubt that the injection of tincture of iron effected the cure in this case, as, although the usual medicines were prescribed at the commencement, nothing more than a simple aperient was given after February 5.—*Medical Times and Gazette, Dec.* 24, 1870, *p.* 727.

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## 33.—ON THE ACTION OF CERTAIN DIURETICS ON THE URINE IN HEALTH.

By Dr. F. B. NUNNELEY, Assistant Physician to the Victoria Park Hospital.

[Dr. Nunneley communicates in this paper the results of a number of experiments made upon himself as to the action of certain diuretics, viz., citrate and acetate of potash, spiritus ætheris nitrosi, and oil of juniper. The following are the conclusions arrived at.]

1. Citrate and acetate of potash only slightly increase the quantity of water excreted by the kidneys.
2. They distinctly lessen the amount both of urea and of solids excreted.
3. Spiritus ætheris nitrosi slightly increases the amount of urinary water.
4. It decidedly reduces the quantity both of the urea and solids.
5. Oil of Juniper slightly reduces the amount of water excreted.
6. It appreciably increases both the urea and the solids.

It would thus appear that, of these four medicines, citrate and acetate of potash and nitrous ether actually *reduce* the urinary solids, whilst they slightly increase the water, and that oil of juniper *increases* the solids whilst it slightly lessens the water.

It is evident that these experiments, performed on one individual, will not establish generally such conclusions, but they at least show that the action of these medicines, with the exception of oil of juniper, as diuretics in *health*, is very uncertain.—*Medico-Chirurgical Trans., vol.* liii., 1870, *p.* 36.

# SURGERY.

## AMPUTATIONS, DISLOCATIONS, FRACTURES, &c.

### 34.—ON SUPRA-CONDYLOID AMPUTATION OF THE THIGH.

By Dr. WILLIAM STOKES, Jun., Surgeon to the Richmond Surgical Hospital, Dublin.

[The main objects of this paper are to point out the advantages of amputation at the knee, and to discuss what is termed supra-condyloid amputation of the thigh. Amputation of the thigh was revived as an operation about forty years ago by Velpeau, who in a rather lengthy paper sets forth its various advantages. These may be briefly epitomised as follows.]

1. That the belief in the risk of exposing large cartilaginous surfaces is proved to be imaginary.
2. That after amputation at the knee the weight of the body can be placed on the face of the stump; whereas in all amputations nearer the trunk the support must be at the tuberosity of the ischium.
3. That after amputation at the knee the hip-joint preserves all its movements, and the patient need not walk as if that joint were anchyloid, as he must do after amputation of the thigh.
4. That the shock is not great, as the wound is almost entirely confined to the integuments, and no muscles of great bulk are divided.
5. That only one artery of any considerable size requires deligation after this mode of amputation.

Fifteen years subsequently, in 1845, Prof. Syme, notwithstanding the rash and indiscriminate proscription of Velpeau's operations (which were stigmatised in one of the leading surgical journals as "experimental murders"), introduced a modification of Velpeau's circular amputation at the knee, this modification consisting in sawing through the condyles and taking a long flap from the calf of the leg. The great advantages of this procedure are, that the medullary canal is not opened, that the amputation is further removed from the trunk, and also that there is less risk of the formation of the tubular sequestra,

which not unfrequently occur after amputation of the thigh, and which result in Mr. Syme's opinion, from injury to the medullary membrane or division of the nutritious artery or arteries. This operation, which appears to be identical with Hoin's, Mr. Syme subsequently abandoned in favour of Mr. Carden's method.

[The principal objection of orthopædic mechanists to this amputation is that the stump is inconveniently long, so that when the artificial knee-joint is added, it does not coincide in relative position with the other knee-joint. This objection, however, might be got over.]

The operation to which I wish to draw special attention in this paper is a modification of the procedure known in some of the Continental schools as "Gritti's amputation," which is itself a modification of Mr. Carden's amputation at the knee. One great difference between my operation and those practised on the Continent is that the femoral section is made considerably higher up, being fully half or three quarters of an inch above the antero-superior edge of the condyloid cartilage. Having made sections of a large number of femora in this situation, I can confidently state that the bone may be divided as high as I have mentioned, and in some cases even higher, without opening the medullary canal.

In the great majority of cases the medullary canal terminates at a point three-quarters of an inch above the cartilage of incrustation on the outside, and over one inch above on the inside, anteriorly. Posteriorly, one half an inch above the articular cartilage on the inside, and more than three-quarters of an inch above it on the outside, as the cartilage of incrustation is higher up in front on the outside and higher up posteriorly on the inside.

In all cases, too, the cartilaginous surface of the patella should be removed, the supposed difficulty of which has been in many persons' opinion a great one, but which, in truth, is quite visionary. The advantages to be derived from dividing the bone so high, are, first, that this method of operating ensures that the portion of the flap to which the patella is attached shall constitute the face of the stump, there being no danger when the femoral section is made so high of the patella being drawn up during the process of healing on to the anterior surface of the femur, as there is in the operation through the condyles; secondly that the divided surface of the femur can be kept perfectly and permanently covered by the patella; and, thirdly, that by making the high section the objection I have already alluded to, of the stump being inconveniently long for the subsequent adaptation of a mechanical appliance, is obviated to a great extent, if not entirely.

In a paper by Dr. Melchiorj, of Salò, on this modification of Carden's operation, a *precis* of which is given in the "Biennial Retrospect of the Sydenham Society" for 1867-8, the particulars of three cases in which this operation was performed are described, but in only one of them was the articular surface of the patella removed. It would appear, therefore, that much importance was not attached by Gritti to the removal of this portion of the bone. It seems to me, however, that in order to get osseous anchylosis between the patella and femur, it is necessary to remove the posterior surface of the former, as the chances of union between the two bones taking place would otherwise be extremely limited.

The anterior flap, not rectangular but oval, should reach from a point one inch above either condyle to the other, and should extend downwards to the tubercle of the tibia. A posterior flap, at least one third of the length of the anterior one, should also be formed, as the posterior tissues retract so much more than the anterior ones; and therefore, unless a flap in this situation be made, there is much risk of a gaping wound resulting, which would greatly protract the convalescence of the patient. In making the posterior flap, the convexity at its anterior extremity should be directed backwards, so that in subsequently adapting the flaps the convexity at the extremity of the anterior should accurately fit the concavity at the extremity of the posterior flap.

It will be seen, therefore, that the differences between this procedure and the Italian operation are—

First. That the femoral section is made in all cases at least half an inch above the antero-superior edge of the condyloid cartilage.

Second. That in all cases the cartilaginous surface of the patella is removed.

Third. That the flap is oval, not rectangular.

Fourth. That there is a posterior flap fully one-third of the length of the anterior flap.

I may now proceed to give as briefly as possible the particulars of the cases in which I performed this operation, and in one of which the amputation was a primary one.

Michael Crow, æt. 32, by occupation a tailor, was admitted into the Richmond Surgical Hospital on the 7th of last January, suffering from extensive carious disease of the bones of the left leg, ankle-joint, and metatarsus. He stated that, about thirteen years previously, while playing at football, he received a kick on the tibia. The leg, he stated, "got sore" immediately, and the disease spread downwards. For more than ten years the disease was localised to the bones of the leg, being confined, namely, to about their middle thirds. The disease then extended

downwards, and about a year after the ankle-joint and metatarsal bones became involved.

On his admission into hospital, the patient had a pale anæmic appearance, and was much debilitated from the long continued and extensive disease. The leg, ankle, and foot were much swollen and œdematous, and numerous sinuses existed leading down to softened diseased bone, through which purulent matter, varying as much in consistence as in quantity, continually oozed. Although there were no sinuses or other evidence of disease in the upper extremity of the tibia, I deemed amputation at the knee preferable to that immediately below the tubercle of the tibia, as from the great length of time the disease had lasted I considered it most probable that the bone in its entire extent was involved, a view which a section of the bone made subsequently verified.

On the 18th January I performed the supra-condyloid amputation in the following manner. I commenced by making an incision with a strong scalpel, beginning one inch above the external condyle, and carried it merely through the integument downwards and forwards to the tubercle of the tibia, and then carried it upwards and backwards to a corresponding point on the inner side of the thigh. The knife was then closely applied to the edge of the somewhat contracted integuments, the deeper structures were separated, and the flap containing the patella rapidly dissected back to a point as high as where the incisions were originally commenced. The posterior flap, at least one third in length of the anterior flap, was then made, the first incision, or that through the skin, being so curved that the convexity should look backwards. The deeper structures were then divided with an ordinary amputation knife, and the rest of the posterior flap completed by this instrument. A transverse section of the femur was then made, commencing half an inch above the commencement of the antero-superior edge of the condyloid articular cartilage. The removal of the articular surface of the patella constituted the last stage of the operation, and was not attended with any difficulty. This section I find is most rapidly accomplished with one of the Lagenbeck's fine resection saws, the so-called "Stichsäge." Only one vessel, the popliteal artery, required deligation; all other arteries were secured by torsion.

I may now indicate what I believe to be the advantages of the supra-condyloid amputation over, first, the amputations through the knee-joint with preservation of the patella, those, namely, of Velpeau, Lane, Blenkins, and Markoe; secondly, the amputation through the condyles, as practised by Syme, Sir W. Fergusson, and others, operations which are closely analogous to, if not identical with, the original operation of

Hoin; thirdly, Mr. Carden's operation, and its modification by Gritti; and lastly, the other amputations of the thigh, in which the medullary canal is necessarily opened.

Many of the advantages of this operation are doubtless common to it, and to the amputations suggested originally by Hoin and Velpeau: for instance, the stumps being more useful for progression in consequence of the possibility of making pressure on its extremity, and the patient not being obliged to walk as if he had anchylosis of the hip-joint, as is always the case when the point of support is at the pelvis instead of at the extremity of the stump. The diminished liability to tubular sequestra is another advantage common to all the amputations at the knee-joint. Again, the operation being further removed from the trunk, makes it less hazardous to the patient. The shock is less than in the amputations higher in the thigh, as the muscles which are divided are few in number, and these are divided, not through their thick fleshy bellies, but at their tendinous extremities. The muscular interspaces, in which suppurative inflammation so often occurs after ordinary amputations, not being opened, the chances of this occurring are at all events diminished—a point which Dr. Markoe strongly dwells on. There is less liability also to suppuration, from the fact that the parts divided in making the anterior flap are only skin and fasciæ. In connection with the diminished liability to suppuration, I would also mention here the fact that the posterior surface of the anterior flap in the supra-condyloid amputation is covered with a natural synovial lining.

In Carden's and all other flap amputations in this situation, the posterior surface of this flap contains the obliquely divided open mouths of innumerable arteries, veins, and lymphatics, and large numbers of which have also fenestrated openings in them as well. This peculiarity of the supra-condyloid amputation must, I am convinced, largely diminish the chances, not only of subsequent exhaustive suppuration, but also of purulent absorption. There is another feature in this operation which I believe must lessen the probability of the pyæmia. Professor Langenbeck has suggested that in amputations of the thigh the cut surface of the bone should, in order to prevent its divided vessels coming in contact with the suppuration in the wound, be covered by a periosteal curtain which, previous to dividing the bone, should be taken from the anterior surface of the femur, commencing about an inch below the point of section. As regards the value of this suggestion for diminishing the mortality of thigh amputations, having only tried it in a very few cases, I am not yet in a position to either verify or disprove Professor Langenbeck's statement on this point. I have, however, tested this plan as well on the human subject as on the

lower animals, and the results of my experiments would tend to show the great liability of the periosteal curtain to slough away, and not become adherent to the cut surface of the bone. In the supra-condyloid amputation, however, we have an osseous curtain covering the cut surface of the femur which never yet has been known to slough away, so that if there be any value in Professor Langenbeck's suggestion the supra-condyloid amputation must have this among its other advantages. The preservation of the portion of the patella, to which the tendon of this powerful extensor of the thigh is attached, has, however, other obvious advantages. These are the increased power of extending the thigh in progression, and rendering the formation of a conical stump impossible. If the section of the patella be not made these advantages do not exist. In the first place because the elevated ridge dividing the posterior surface of the bone into two unequal portions effectually prevents the divided surface of the femur and patella being in perfect contact. Again, unless osseous anchylosis takes place, there can be no standpoint or firm point of fixation for the extensors to work on; and, lastly, there will always be liability to exfoliation or necrosis of the cartilage of the patella.

Lastly, there is an advantage in the supra-condyloid operation which is possessed by the ordinary circular method of amputation, and which, among other reasons, makes so many surgeons, myself among the number, in amputations of the arm and thigh cling to the circular method in preference to the ordinary flap operations, and that advantage is that the vessels are divided at right angles to their continuity, and not obliquely as they are in all flap operations, which must render these vessels more liable to take on inflammatory action from the wounds in them being necessarily so much greater in extent. I think, therefore, I may fairly claim for the operation which has been the subject of this communication, among other advantages that I have already mentioned, those of both the circular and flap amputations and the defects of neither.—*Medico-Chirurgical Transactions, vol.* liii., 1870, *p.* 175.

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## 35.—ON AMPUTATION AT THE KNEE-JOINT.

By GEORGE POLLOCK, Esq., Surgeon to St. George's Hospital.

[Since 1830, when Velpeau advocated its adoption, the operation of amputation through the knee-joint has been steadily gaining in favour.]

It must be borne in mind that there are two different and distinct conditions involved in the proposal to remove the leg at the knee-joint: 1st. If there be disease of the joint with ulcera-

tion of the cartilages, amputation should be completed by the removal of the condyles of the femur and the articular surface of the patella, or the whole of that bone. 2nd. If there be no disease of the knee, but, from disease or accident, amputation of the leg be requisite, it should be completed by cutting through the joint without interference with the articular surfaces of the femur or patella.

Mr. Syme was one of the first in this country to encourage surgeons to view this operation as practical and comparatively safe. In 1845, he drew attention to the great mortality attendant on amputation through the thigh, and to the inconveniences of the stump which followed, often irritable, usually uncomfortable, occasionally much retracted and conical, and seldom or never capable of bearing any pressure on its extremity. Instead, therefore, of cutting through the thigh and the shaft of the femur, he amputated the limb at the joint, and removed subsequently a portion of the condyles. He justly observes that "the warrant for amputation (in diseases of the joint) lies in the bone," and not in the soft parts. By cutting through the condyles, instead of the shaft of the femur, he expected to avoid the risk of necrosis; and by cutting through the cancellated structure of the condyles, he hoped to escape those dangers so commonly attendant on opening the medullary canal. He concluded that amputation through the condyles would ever prove less fatal than amputation through the shaft of the bone, and subsequent experience tends to prove the soundness of his views.

I must here remark that whatever can be urged in favour of amputation through the condyles, may be adduced as equally satisfactory, if not as stronger evidence in behalf of amputation through the joint, without removal of the articular cartilages of femur or patella; and I trust I am justified in advancing this opinion on the strength of certain facts which are presently to be considered.

Mr. Syme, at the time above alluded to, does not appear to have made any reference to—he certainly did not then advocate—the more simple operation to which I refer. Mr. Samuel Lane, of St. Mary's Hospital, was I believe one of the first surgeons in this country to perform this operation. In 1857 he successfully amputated through the knee-joint, without removing the articular cartilage, or the patella; and so satisfied was he with the operation that he has repeated it in several instances, and with very satisfactory results.

[Since 1864, the opportunity has occurred to Mr. Pollock of performing this operation eight times. Of these cases one

died, a lunatic, and a most unfavourable case for operation. The result in all the successful cases was that the patient could bear the weight of the body well upon the extremity of the stump. This is not generally the case in the ordinary amputation through the shaft of the femur.]—*Med.-Chir. Transactions, vol.* liii., 1870, *p.* 1.

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## 36.—MODIFIED CIRCULAR AMPUTATION OF THE FOREARM.

By Dr. G. H. B. MACLEOD, Professor of Surgery in the University of Glasgow.

In all the flap operations of the forearm there is great risk of the bones projecting at the point of union of the flaps, and this is especially the case in the lower portion of the limb, where the shape is so flat.

The method I adopt is a modification of the circular. The main objections to the circular in the forearm are the difficulty of retracting the flaps (from the conical shape of the limb) so as to allow of the bones being sawn high enough to secure a good covering and further the deep hollow left for the retention of pus. Both of these objections are overcome by making a longitudinal incision on the lower (ulnar) side from the free edge of the flap to within an inch or so of the point where the bones are to be divided. By such means complete and constant drainage is secured, the retraction of the flaps is rendered easy, and another step in the management of the stump, on which I put great weight, is rendered feasible. I allude to the application of a long pad of lint doubled over the end of the stump (after the edges of the flap have been placed carefully in apposition by sutures), and extending on either surface as high as the point of section of the bones, and clasped together (not too firmly, however) by two metal sutures passing through the flaps. In this way the soft parts are held accurately together throughout their whole internal surfaces, no hollow is left within, and yet the dependent side-cut allows the free escape of all secretion. The bones are deeply covered and cannot project; and, as a rule, a rapid closure of the wound, with a firm, fleshy stump results. A piece of wet lint thrown loosely over the stump is the only dressing used, so that the progress of matters can be constantly observed.

I may here remark that the circular method of amputating does exceedingly well in the lower half of the thigh, if a longitudinal cut be made on the under surface for the escape of secretions.—*Glasgow Med. Journal, Nov.*, 1870, *p.* 3.

## 37.—ON SUBCUTANEOUS DIVISION OF THE NECK OF THE THIGH BONE.

By WILLIAM ADAMS, Esq., Surgeon to the Royal Orthopædic and Great Northern Hospitals.

[Anchylosis of the hip-joint, for the relief of which the operation of division of the neck of the femur subcutaneously is now under trial, may be true or false. True anchylosis is of course a bony union, and is generally the result of strumous disease with ulceration of cartilages.]

The first operation, having for its object not only that of rectifying the deformity, but of obtaining motion by the establishment of a false joint, was performed by Dr. Rhea Barton, of Philadelphia, United States, in 1826. This operation was accomplished by a crucial incision made over the great trochanter, seven inches in length and five inches in a horizontal direction. The bone was then divided transversely by a fine saw—it is said "between the two trochanters"—probably just above the small trochanter. The natural direction of the limb was at once restored, and the case proceeded favourably. It is said that useful motion was obtained, but that seven years afterwards anchylosis took place, and that the man died of phthisis nine years after the operation.

The next operation worthy of attention is recorded by Dr. Louis Sayre, of New York, who operated successfully on two cases in which he performed a new operation, which he had proposed for obtaining a false joint and preserving motion in cases of bony anchylosis of the hip-joint, with the thigh in a flexed position. The theory of this operation was to obtain free motion by the formation of a false joint, of a ball-and-socket character, supposed to resemble the hip-joint in possessing an actabulum or cavity corresponding to this, and a rounded extremity of bone corresponding to the head of the femur; and also a round ligament.

The operation consisted in the removal of a transverse section of the femur, of elliptical form, just above the trochanter minor by means of the chain-saw, an incision of about six inches in length being made over the trochanter major in the axis of the limb. The first patient, Robert Anderson, aged 26, was operated upon on the 11th June, 1862, and in December of the same year he is reported as follows, "Could stand on either leg without either crutch or cane"; and as late as April 29th, 1868, Dr. J. S. Green says, in a letter to Dr. Sayre, that "Robert Anderson still lives, moves, and walks with practical agility" (p. 35, pamphlet.)

The second operation was performed 6th November, 1862, on Miss Susan M. Losee, aged 24. This case proceeded less favourably than the first but all discharge from the wound ceased four

months after the operation. Subsequently, however, an abscess formed, and a little necrosed bone escaped. Pneumonia and pleurisy occurred, and she died on the 17th May, 1863. At the *post-mortem* examination, tubercular deposits were found in the lungs, and a large abscess in the left lung. The artificial joint was found to be provided with a complete capsular ligament, and the articulating surfaces were tipped with cartilage and furnished with synovial membrane. In consequence of Dr. Bauer, in his work on *Orthopædic Surgery* (p. 325), stating that this case of Dr. Sayre's died of pyæmia, a number of letters from medical men are given in the appendix to the paper, confirming the tubercular theory, and also Dr. Sayre's statement as to the existence of cartilaginous covering to the bone, synovial membrane, &c.

I am not aware of any operation having been performed in this or any other country on the hip-joint in cases of bony anchylosis, with the object of obtaining free motion by the establishment of a so-called "artificial joint"; and it will at once be seen that further experience is required before we can confidently speak of the success of such an operation, although the cases recorded by Dr. Sayre are undoubtedly worthy of the most attentive consideration.

It occurred to me, however, that in these cases of bony anchylosis of the hip-joint, with extreme distortion, a much more simple operation might be performed by the subcutaneous division of the neck of the thigh-bone, about its centre, within the capsular ligament, using for this purpose only a long tenotomy-knife, and a very small saw constructed for the purpose, with an inch and a half cutting edge, and a long thin shank like a tenotomy-knife; and on the 1st December, 1869, I performed this operation successfully on the following case.

Luke Bristowe, aged 24, a gardener from Loudon, near Chippenham, Wiltshire, was admitted into the Orthopædic Hospital on the 12th October, 1869, in consequence of extreme deformity at the hip-joint. The thigh was flexed upon the pelvis at a right angle, and firmly anchylosed in this position; the heel of the right leg rested on the upper part of the left knee-joint, and the limb was therefore perfectly useless. The only mode of progression was either with two crutches, or with one crutch and a stick, which he generally used. He had also anchylosis of the vertebral articulations through a considerable portion of the spinal column; all the lumbar and lower dorsal vertebræ were perfectly immoveable, and the spine was curved posteriorly, with an inclination to the right side in the lower dorsal and upper lumbar region. In consequence of this anchylosis through the lumbar region, the pelvis and spine could only be moved together, and the trunk and leg therefore ap-

peared to be remarkably fixed in the deformed position. There was also a fixed and permanent obliquity of the pelvis, with regard to the spinal column, to the extent of two inches, as ascertained by careful measurement. Partial anchylosis also existed in some of the upper cervical vertebræ; the motion between the occipital bone and the atlas was free, but between the atlas and the axis motion was extremely limited, and the head was habitually carried forwards. This affection was the result of an extremely severe attack of rheumatic fever, with which he was seized seven years ago June last, and which, he stated, was not preceded by gonorrhœa. Various articulations were affected during the fever, and the rheumatic pains were severe for six months, and continued more or less for twelve months. During the latter part of this time he was an inmate of the Bath Hospital for fifteen weeks, and had the hot mineral baths, but without any marked relief; and he then went into the Brighton Hospital for nine weeks, where he was galvanised, and had to swing a seven-pound weight, but without material benefit. No treatment had been adopted during the last six years, nor had he suffered from any further attack of rheumatism. His general health was good, and also his family history.

That the case was one of true bony anchylosis, was proved by the failure of forcible extension under chloroform, tried on three separate occasions at the Orthopædic Hospital; and after this I suggested to the patient the operation of dividing through the bone as the only means of restoring the position of the limb, and he readily gave his assent. For the purpose of the operation, and that he might have the advantage of superior nursing, if required, he was removed to the Great Northern Hospital on the 26th November, 1869.

On the 1st December, 1869, I divided the neck of the thigh-bone subcutaneously within the capsular ligament, in the presence of my colleagues, Mr. Gay, Mr. Carr Jackson, and Mr. Shillitoe; Mr. Mason, Dr. H. Dick, and Mr. John Mackenzie of the Bombay Hospital, were also present. The instruments used were a long tenotomy-knife, and a very small saw, three-eighths of an inch in width, and with a cutting edge an inch and a half in length, at the end of a slender shank three inches in length, made by Mr. Blaise of St. James's Street. The details of the operation were as follows.

I entered the tenotomy-knife a little above the top of the great trochanter, and, carrying it straight down to the neck of the thigh-bone, divided the muscles and opened the capsular ligament freely. Withdrawing the knife, I carried the small saw along the track made—preserving this by pressure of the fingers—straight down to the bone, and sawed through it from

before backwards: this was accomplished in five minutes. No hemorrhage followed; and I immediately applied a compress of dry lint, a plaster, and bandage.

As soon as the bone was cut through, the leg moved freely in all directions; but, before it could be brought into a straight position, it was necessary to divide the tendons of the long head of the rectus and of the adductor longus muscles, and to cut through the tensor vaginæ femoris muscle. The limb was fixed in a straight position and bandaged to a long interrupted Liston's splint. No inflammation, whatever followed the operation; no swelling or redness of the skin, or any deep suppuration; but the wound healed slowly. The House-Surgeon, Mr. Willis, reports as follows.

Dec. 4th. The long splint was changed to-day for a short one. He could move the leg from the hip gently whilst the splint was off. The tenotomy wounds were quite healed.

Dec. 5th. A four-pound weight was attached to the leg, which he bore well for a time.

Dec. 7th. The dressing was removed for the first time to-day. A few drops of pus only escaped from the superficial wound. There was no deep suppuration going on. The superficial wound was dressed with carbolic lotion, one part in forty.

Dec. 13th. The splint was removed altogether to-day. He could draw the leg up almost as well as the sound one, and had very fair motion at the hip.

Dec. 22nd. He was going on well. No febrile symptoms or deep suppuration were going on. The superficial wound was nearly healed. There never had been more than two or three drops of pus on the lint in the morning. Collodion and castor-oil were applied to-day instead of carbolic acid lotion. He got up to-day for the first time, just three weeks since the operation.

I encouraged motion from the 13th December, and moved the limb frequently myself; and, when he walked about the ward on crutches, induced the patient to swing the leg as much as possible. After walking about daily for a fortnight, however, the limb began to stiffen at the hip, and all attempts at movement were painful. I then determined to abandon all idea of obtaining motion, and endeavour to procure bony anchylosis with the limb in a straight position.

On the 6th January, 1870, the man was ordered to keep his bed, and the limb to be maintained in a straight position by an extending weight of from three to five pounds suspended over the end of the bed. At the end of three weeks, the divided neck of the femur seemed to be firmly anchylosed; and on the 24th January he was discharged and transferred to the Royal Orthopædic Hospital.

March 18th. He had gained sufficient strength to be able to

walk about the ward with the aid of one stick, and he could walk a little without any assistance.

April 25th. He was taken to the Medical Society of London, and exhibited his power of walking about the room without any assistance. He continued steadily to improve, and gained sufficient strength to bear the entire weight of his body on the leg which had been operated upon. He still, however, generally used one stick in walking, and walks with the body somewhat inclined to the right side in consequence of the obliquity of the pelvis and anchylosis of the lumbar and lower dorsal vertebræ. Although there appears to be a little shortening of the right leg, he is not at all improved, as to the erect position of his body, by any addition to the boot, so that he wears boots of the same thickness.

Nov. 10th, 1870. This patient having been at his home in the country for several months, came to London, and I again examined him. He could now walk three or four miles with ease. and did not require a stick for walking purposes, although he always used one to improve the general balance of his body, which was somewhat disturbed by the spinal curvature.

In comparing the relative merits of the three operations which I have now described for rectifying extreme distortion at the hip-joint with bony anchylosis, the different objects sought to be accomplished and the risk to life incurred in each operation must be borne in mind. The operations performed by Barton and Sayre were undoubtedly of a formidable character, requiring large external incisions, and necessitating considerable disturbance of structures at a great depth from the surface to allow of the use either of an ordinary saw or of a chain-saw, as employed by Sayre; and the evidence is not yet sufficient to prove that even by such means a permanently useful artificial joint can be established in the neighbourhood of the hip-joint. In Rhea Barton's case, bony anchylosis was proved to have taken place by the *post mortem* examination of the patient, who died from phthisis eight years after the operation, although motion is said to have been preserved for six years.

In Sayre's first case, in which a segment of bone was removed above the small trochanter, an useful limb was permanently obtained, and good motion existed five months after the operation, but some necrosis subsequently occurred; and the late accounts, six years afterwards, are less satisfactory than we could desire as to the evidence of free motion at the joint. In Sayre's second case, which undoubtedly offers a good illustration of the establishment of a false joint, necrosis, although in a limited degree, was still proceeding at the time of death—six months after the operation—and bone had exfoliated previously,

so that Dr. Bauer was induced to believe that the death arose from pyæmia rather than phthisis.

In the case which I have now brought before the meeting, I was encouraged, by the absence of inflammation, to hope for the establishment of motion; but, this failing, the result was limited to remedying the deformity and obtaining a useful limb for the patient, with bony anchylosis in a normal position; and to such a result I would advise, in all future operations, that our expectations should be limited.

With regard to the subcutaneous operation, which, so far as I know, was first suggested and performed by myself, and which I have now brought before the British Medical Association, I would only observe that the subcutaneous division of bone—subcutaneous osteotomy, as it may be called—has proved itself to be as simple and harmless an operation in its immediate effects as subcutaneous tenotomy, with which, in its essential characters, the operation may be compared.

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Since this paper was read at the Association meeting, this operation has been successfully performed by Mr. T. R. Jessop of Leeds, who on the 17th September, 1870, writes as follows. "Three weeks ago last Thursday, I performed the operation upon a young woman whose right thigh was fixed by bony anchylosis at a right angle with the pelvis. By practising upon the dead subject, I had previously found that the best spot at which to make the puncture was about an inch behind the posterior margin of the great trochanter, near its upper border. The division we made without any difficulty. The operation and the subsequent dressings were performed after Professor Lister's method. The wound healed at once without a single drop of pus. For a fortnight I kept the patient in bed, with the limb stretched by means of a weight slung over a pulley attached to the ankle. Since the expiration of a fortnight, I have had the woman up, walking about on crutches, and swinging the leg in all directions, with the view of procuring a false joint. The limb measures barely an inch shorter than the sound one; and the patient makes complaint only of a little pain in both hip and knee. Crepitus can both be felt and heard very distinctly. I will have the result duly reported." The operation has also been successfully performed by Mr. F. W. Jowers, of Brighton, and Mr. Furneaux Jordan, of Birmingham. No inflammation or suppuration followed the operation in either case. The details of these three successful cases will shortly be published; and the fact that this operation has been successfully performed in four cases, goes far to establish it as a surgical procedure.—*British Med. Journal, Dec.* 24, 1870, *p.* 673.

## 38.—SUBCUTANEOUS SECTION OF THE NECK OF THE FEMUR.

By FURNEAUX JORDAN, Esq., Surgeon to the Queen's Hospital, Birmingham.

Emma H., aged 16, from Wales, had had hip-disease for six years. She had had several sinuses opening and closing during that time. On admission into hospital, there was a little oozing from one near the perinæum. The thigh was flexed at right angles to the trunk; and there was unmistakable osseous anchylosis of the hip-joint, as revealed by examination under chloroform. I divided the neck of the femur by the method and with the instruments devised by Mr. W. Adams. The sudden mobility of the joint when the section of the femur was completed was very striking. The adductor longus and long head of the rectus femoris required tenotomy. The limb was then put into a position which promises a very useful result. It is now three weeks since the operation, and the progress has been most favourable.

In this case, the naturally short femoral neck of early life, made shorter by caries and anchylosis, combined with a very fat gluteal region (since anchylosis, the patient has become very stout), required more than ordinary care in every step of the operation.—*British Med. Journal, Dec.* 24, 1870, *p.* 676.

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## 39.—A NEW METHOD OF UNITING FLAPS BY DEEP SUTURES.

Mr. Couper, of the London Hospital, who has for some time past practised Professor Lister's system of antiseptic treatment of wounds with very good results, has, quite recently, combined with it the following means of securing apposition of the deep portions of flaps. A curved needle, into which is threaded a large carbolised gut ligature, is introduced into the flap, close to its edge, and a short distance from its base; the needle is passed in a semicircular course, transversely to the axis of the limb, immediately beneath the skin, until it emerges at a corresponding point on the opposite side of the flap; the thread is then drawn through to within two or three inches of its end. The needle is next introduced into the other flap, at a point opposite to where it emerged from the first—the point which, as far as can be judged, will correspond to it when the two surfaces are brought into contact,—and is made to travel in a similar transverse semicircular direction, so that in the end it emerges in the one flap at a point opposite and corresponding to the one where it was first introduced into the other, having completed a circular course beneath the skin of the two flaps.

The ends of the suture are drawn until the flaps are brought into accurate contact (much as the mouth of a bag is closed by drawing the ends of a circular string), and knotted and cut off short. If the flaps are long, another suture is passed parallel to the first, a little further from their bases; and when the skin edges have been secured with another suture, the one or more circular ones, knot and all, are completely included and shut out of sight. Before the external suture is quite secured, a syringe is insinuated between the flaps at the spot which remains open, and a jet of carbolised fluid is injected to ascertain whether there remains sufficient egress for any fluid that may be poured out from the wounded surfaces. In the first cases Mr. Couper employed for the external suture a fine gut thread, which he tied interruptedly, so as to have an abundant opening for discharges; but, as he found it to give way before sufficient union had taken place, he has resorted to the use of metal sutures; and, as the precaution he employed for the free discharge of matter proved to be unnecessary, he has used iron wire in his later cases, and applied it in a very close uninterrupted stitch, which is made to include the skin only.

A few days since we saw a case in which Mr. Hutchinson had amputated the forearm at about the middle, and in which Mr. Couper had, at the operator's request, undertaken to bring the flaps together in the manner above described, with all the precautions of Lister's system of dressing. It was the ninth day since the operation; the situation of the union of the flaps was indicated by a thin line of dried sanguineous matter, altered in colour by the carbolic dressing, beneath which lay the finely stitched continuous suture, which, as nothing but the extreme edge of the skin had been included, would, in the course of two or three days more, cut its own way through, and separate itself without external aid. We ascertained that, with the exception of a very slight serous oozing during the first twenty-four hours, no discharge whatever had been thrown out; nor has there been in any of Mr. Couper's later cases which have been similarly treated. In some cases he finds it useful to employ in addition one or two gut sutures, inserted by transfixion of the flaps, and tied externally. These wither in the course of two or three days, at the point where they enter the skin; and when brushed away leave a small red cicatrix of about the same diameter as the suture employed.—*Lancet, Dec.* 24, 1870, *p.* 886.

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### 40.—TRIPOD APPARATUS FOR THE REDUCTION OF DISLOCATIONS OF THE HIP.

The following is abstracted from a monograph on the hip-joint by Dr. Bigelow of Massachusetts, U.S. The patient is to

be laid on his back on the ground and the pelvis is to be buckled to the floor; and the limb, flexed at the knee and hip, is suspended by cord and pulleys from the leathern cap which fits on the summit of a tripod. A transverse rod passes across the front of the knee through rings in the pair of angular splints. The projecting ends of this rod afford the means of effecting powerful rotation of the femur; and a similar rod, placed behind the leg and in its axis, supplies the means of producing "another useful movement, call by the French *bascule* or tilt." By vertically raising this rod at its upper extremity, Dr. Bigelow holds that the head of the bone may be carried from the dorsum or pubes in the direction of the tuberosity: "Although the need of this apparatus may be rare, it will prove occasionally efficient in reducing a luxation of long standing or complicated with fracture. At any rate, I cannot believe that the period is remote when longitudinal extension by pulleys to reduce a recent hip-luxation will be unheard of." We fully agree with the author as to the importance of the flexed position, and would suggest that the tripod part of his apparatus might easily be dispensed with in cases in which the roof of the room, or a doorway, affords a convenient beam for the reception of a strong hook. The rest might also be extemporised by any one who understood its principles.—*British Med. Journal, Jan.* 14, 1871, *p.* 38.

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## 41.—ON A CASE ILLUSTRATING THE PRESENT ASPECT OF THE ANTISEPTIC SYSTEM OF TREATMENT IN SURGERY.

By JOSEPH LISTER, Esq., F.R.S., Professor of Clinical Surgery in the University of Edinburgh.

A young man, eighteen years of age, was lately admitted under my care in the Royal Infirmary on account of impaired usefulness of the right arm, resulting from an accident which befell him three months previously, when the handle of a winch, revolving with great rapidity, struck the limb at the posterior aspect, about three inches below the elbow, breaking the ulna and dislocating the upper end of the radius forwards, the lower ends of the bones of the fore-arm being tilted backwards to a corresponding degree. He at once sought medical aid; but, strange to say, the nature of the injury was not recognised, and the result was that when I saw him the fragments of the ulna were firmly united at an obtuse angle with each other; a marked depression existing posteriorly over the seat of fracture, while the head of the radius formed a prominence at the anterior and outer aspect of the joint, being securely maintained in its abnormal position through the connection of the other

end of the bone with the lower end of the ulna. The elbow could not be flexed beyond a right angle, so that he could not put his fingers to his mouth; and, although the hand could be rotated passively, he was quite unable himself to execute pronation or supination. He also complained that the limb was so weak that he could not lift any heavy object from the ground, and expressed great desire to have this faulty state of matters rectified.

It was plain that before an attempt at reduction of the dislocated radius could be made with any chance of success, it would be necessary to break again the united ulna. But, considering the length of time that had passed since the accident, and the slightness of the leverage that could be obtained upon the seat of fracture so near the elbow, it seemed hardly likely that this object could be attained without a cutting operation. And even supposing the bone to give way, I felt it very doubtful whether the dislocation could even then be reduced, both on account of its long duration and because the angular form which the ulna had assumed implied a shortening of the forearm, which at that late period necessarily affected all its textures. On the other hand, there could be little doubt that, if the ulna were exposed and divided, and if, further, the head of the radius were removed, the limb could be at once restored to its proper form. But to do this would be to make voluntarily a compound fracture of the ulna and a compound dislocation of the elbow-joint—a procedure which, under ordinary treatment, I should have regarded as unjustifiable. But with the means now at our disposal of guarding against the mischievous influence of external agents upon wounds, I believed that these two operations could be performed without any chance of mischief resulting. Accordingly, at a clinical lecture on the 12th December, having explained the aspect of the case, and having failed to rebreak the ulna by very forcible measures under chloroform, I first washed the skin of the fore-arm and elbow with 1 to 20 watery solution of carbolic acid, to destroy all putrefactive particles in the epidermis and hair-follicles, and then made a longitudinal incision about two inches long over the back of the ulna where it had been broken, while an assistant threw over the part a cloud of spray of 1 to 40 carbolic lotion by means of Richardson's apparatus; and, having sufficiently detached with the knife the muscles from the bone, and ascertaining precisely with the finger the situation of the callus, I inserted the blades of a pair of strong bone-pliers, smeared with an oily solution of the acid (1 to 10), and, cutting through the bone, used the pliers as a powerful lever to wrench the fragments sufficiently apart, and detach them enough from surrounding soft parts to insure free mobility, the antiseptic spray

being meanwhile constantly maintained. A sponge wrung out of 1 to 40 watery solution having then been bandaged upon the wound, I made an attempt to reduce the dislocation of the radius; but, meeting with the failure I had anticipated, I at once cut down upon its head in a cloud of spray, and removed it by nipping through its neck with the pliers, the blades of which had been again smeared with the oil. A folded cloth dipped in the watery solution having been laid upon the wound, I had the satisfaction of finding the fore-arm assume, under moderate extension and coaptation, a perfectly normal shape. The limb was then enveloped in lac-plaster from the middle of the arm to the lower part of the fore-arm, the sponge and cloth having been previously removed under the spray, the wounds being left unstitched, to secure complete absence of tension from accumulating blood or serum. Cloths to absorb discharge, and a roller smoothly applied so as to adapt the plaster well to the limb, and a pair of Gooch's splints, anterior and posterior, with a special pad in front over the seat of fracture, completed the dressing, the elbow being kept at a right angle.

Next day, the dressings were entirely changed, when it was found that a good deal of blood and serum had oozed into the cloths. The lac-plaster was cut up with scissors along a line distant from the wounds; and, as it was raised from the limb, the spray of 1 to 40 lotion was made to play beneath it. The gaping wounds were found filled with blood-clot, while the limb was free from swelling, redness, or tenderness. The limb having been washed from bloody stain with a cloth dipped in 1 to 40 lotion, while the wounds were kept covered with bits of rag wrung out of the same, oiled silk "protective," dipped in the lotion to give it a temporary antiseptic film, was placed upon each wound to protect it from the stimulating action of the acid in the lac-plaster, which was then wrapped round the limb in two layers, extending several inches beyond the "protective" in every direction, after which the splints were reapplied as before.

This dressing was left unchanged for two days, after which the patient was again dressed in a precisely similar manner before the clinical class, walking into the theatre and upstairs again to his bed just like a person affected with a simple fracture or dislocation. His pulse was 70, his temperature 98·2, and he was entirely free from pain. The stain on the cloths corresponded to about half a drachm of bloody serum; the clots remained unaltered in appearance in the wounds, and the limb in the vicinity had still a perfectly natural aspect. Feeling sure that the discharge would now be very slight in amount, I left this dressing untouched till the following lecture, four days later, or just one week after the operation, when the wounds were

again exposed before the class. All remained the same, except that while there was no pus, and merely a stain corresponding to a few minims of serum as the product of both wounds for four days, the blood-clots had been extensively converted into vascular tissue, while some portions yet unvascularised had assumed a grey or yellowish colour, and in both wounds there was a broad cicatrising margin. Healing, though under a moist dressing, was going on as under a scab; or, in other words, putrefaction being excluded by means of an efficient antiseptic guard, while the exposed tissues were protected from the action of the antiseptic salt by the interposition of a layer of unstimulating material, the disturbing influence of external agency was avoided, and we attained very closely to the conditions of a subcutaneous injury.

On this occasion, instead of the lac-plaster, a folded muslin cloth, of open texture, imbued with a mixture of paraffin, resin, and carbolic acid, was employed to combine the functions of the lac-plaster and absorbing cloth. Hitherto I have been opposed to porous antiseptic dressings, having observed that, when in the form of lint steeped in an oily solution of carbolic acid, the discharge, if at all free, washed out the antiseptic liquid from among the neutral fibres, and opened a way for the penetration of putrefaction. But, having heard reports from various quarters of the efficacy of oakum, I have lately put it to the test with granulating sores, where, if it should happen to fail, no mischief would result, and I have found it more than answer my expectations. The reason for its superiority over oily cloths is readily intelligible. Each fibre of the oakum is imbued with an insoluble vehicle of the antiseptic; so that the discharge in passing among the fibres cannot wash out the agent any more than it can when flowing beneath the lac-plaster, to a narrow strip of which an individual oakum fibre is fairly comparable. I may remark as worthy of notice by those who still cling to the idea that carbolic acid has some unknown virtue distinct from its antiseptic property, that oakum contains none of that substance, but creosote and probably other antiseptic hydrocarbons, the effects of which in preserving smoked meat are familiar.

Oakum not only proved efficient antiseptically, but presented several advantages over lac-plaster. When the latter is left as a dressing for several days together, the discharge, even though small in amount, soaking into the absorbing cloths, loses the carbolic acid it had received from the plaster, and putrefying from day to day, assumes an acrid character, and sometimes produces most troublesome irritation of the skin. This is, of course, avoided by the oakum. Again, the lac-plaster being quite impermeable to watery fluid, keeps the skin beneath it

moist, and, in fact, covered with a weak watery solution of carbolic acid, which, I suspect, insinuates itself, more or less, beneath the protective, and maintains a slight stimulating influence upon the parts beneath it. But oakum, draining away the discharge as fast as it is effused, avoids this source of disturbance. The result is, that if a granulating sore is thoroughly washed with an antiseptic lotion and covered with "protective" and a well-overlapping mass of oakum secured with a bandage, a dressing is provided which nearly approaches the ideal I have long had in view. For, as granulations do not form pus or even exude serum except when stimulated, a persistent antiseptic, combined with an efficient protective, should constitute a more or less permanent dressing under which discharge should cease and cicatrization proceed with great rapidity. Accordingly, ulcers of the leg treated in this way have been found, when exposed after the lapse of several days, either entirely healed or greatly advanced in the process, while the moisture beneath the protective has been of a serous character and the discharge collected in the oakum comparatively small in amount. Lastly, the lac-plaster has this further disadvantage from the moisture beneath it, that it prevents efficient strapping in cases that require it. But under oakum an adhesive plaster retains its hold as well as under dry lint.

But while oakum has these great advantages, it is disagreeable to many persons from its strong tarry smell; and I have been lately endeavouring to apply the oakum principle in some shape free from this objection. Oakum consists of the detached fibres of old ropes which had been treated with Stockholm tar, among the constituents of which is common resin. I happened to notice, several years ago, that resin holds carbolic acid with remarkable tenacity, so that if one part of the latter be mixed by melting with five of the former, the glutinous mixture which results on cooling communicates only a slight warm taste to the tongue, though containing so large a proportion of the pungent antiseptic, But this material is of itself too sticky for the purpose, and resin is, besides, somewhat irritating to delicate skins. Paraffin, another constituent of tar, is remarkable for its entire absence of adhesiveness, as well as for its perfect blandness; but when pure, though it may be mingled with carbolic acid in the melted state, it separates entirely on cooling. If, however, the three ingredients be melted together, the resin, though intimately blending with the paraffin, still retains its hold upon the acid after cooling, and by a proper proportion between them, a product is obtained which, while intermediate in physical properties between the glutinous resin and the powdery paraffin, is unirritating to the most sensitive skin and highly retentive of the acid, while almost destitute of odour.

Cheap muslin gauze dipped in the melted mass, and well wrung or pressed while hot, is an elegant and convenient form of modified oakum. It should be folded into about eight layers; and in order to prevent the discharge from soaking too directly through it, a piece of thin gutta percha tissue may be placed beneath the outer layer to guide the fluid towards the edge of the cloth.

Such was the dressing employed a week after the operation. Three days later, the wounds were found still healing rapidly without suppuration, and, on rotation of the hand, the end of the radius was felt moving in its proper place, while the ulna presented a slight convexity backwards, instead of its old concavity. The patient who had been till then confined for the most part to bed, as a matter of precaution, was now allowed to get up, a similar dressing of "protective" covered with antiseptic gauze having been applied.

Four days afterwards, on the 26th instant, the dressing was again changed, when the wound over the ulna was found almost healed, and that over the joint far advanced in cicatrisation, while there was still no pus or putrefactive odour, and the general health of the patient continued excellent.

In some respects it would have been more satisfactory if sufficient time had passed to permit reunion of the ulna, so that the usefulness of the limb might be tested. But as an illustration of antiseptic treatment, the case is already complete. In this respect, I cannot but hope that it will be thought instructive. It is an example of a procedure, otherwise highly dangerous, if not unwarrantable, rendered not only legitimate, but entirely free from risk, simply because, from the circumstances of the case, and the improved means at our disposal, we could calculate with certainty on avoidance of putrefaction. I venture to draw special attention to the use of the spray. In every wound treated antiseptically, two things are always to be attended to: first, to leave the wound free from living putrefactive organisms, and, second, to employ such an external dressing as shall securely prevent the entrance of such organisms at any subsequent period of the case, The latter point has, in most cases, been for a long time past satisfactorily accomplished; but the former, till we used the spray, was always a matter of more or less uncertanty. A floating germ might enter during the operation into some cellular interstice among the tissues, and, becoming surrounded with a clot of blood, might escape the action of the antiseptic lotion with which the wound was washed, and, retaining its vitality, might subsequently propagate its kind, and spread putrefactive fermentation through the wound. But by help of the spray we operate in an antiseptic atmosphere, and effectually prevent putrefactive organisms from ever entering the wound alive. We thus dispense with the

necessity for washing the wound at all with an antiseptic lotion, and in the particular case above related, not even the vapour of carbolic acid penetrated into the deeper parts of the wound, which were thus left as free from irritation as if they had been made subcutaneously.

The spray is also of the greatest value during the stitching of such wounds as require it, and rids us of the troublesome and uncertain process of distending the wound with lotion by means of a syringe, after the introduction of the last suture. In the changing of dressings, also, the spray is in some cases, and especially in stumps after amputation, a great element of security.

Revision of the proof (Jan. 11, 1871) affords me the opportunity of giving another report of the progress of the case. On dressing the limb yesterday, after an interval of five days, I found the ulnar wound entirely healed, while at the site of the radial incision two or three granulations about as large as pins' heads alone remained to cicatrize, and an odourless serous stain of about a minim upon the gauze was the only appearance of discharge. The ulna seemed already firmly united; and, after performing passive motion throughout the range of the natural movements of the joint, I directed him to try its powers. He could himself pronate and supinate the hand, could extend the arm completely, and readily put his fingers to his mouth; and he lifted a heavy pair of tongs, exhibiting already a strength very superior to that which he had before the operation.—*British Med. Journal, Jan.* 14, 1871, *p.* 30.

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## 42.—ON THE APPLICATION OF CARBOLIC ACID AS A LOCAL ANÆSTHETIC IN SURGICAL OPERATIONS.

By Dr. J. H. Bill, Surgeon U. S. Army.

In conducting some investigations on the action, &c., of carbolic acid, the writer has made the following observations, which, as they seem to be of practical importance, he communicates in advance of the other results:—

All who have handled this substance must have noticed the tingling sensation (not unlike that produced by aconite) in the finger tips and other parts touched by the acid, which presently passes into a greater or less anæsthesia. On trying to determine the amount of this anæsthesia with an ordinary æsthesiometer it was found not only impossible to distinguish two points, however widely separated, but even to recognise the presence of one.

The prick of the point was not felt at all as pain, nor was an incision productive of uneasiness. The experiment was, therefore, extended thus:—The radial side of the writer's left fore-

arm was covered with a cloth soaked in a saturated solution for a half hour, then a streak was traced over the course of the radial artery with a camel's hair brush dipped in acid liquefied by one-twentieth bulk of water. This streak extended from the styloid process to near the elbow, and after a few minutes was rubbed off. An incision was then made with a common scalpel from a point about two inches above the styloid process towards the internal condyle for five inches, occupying as near as possible the middle of the streak made with the brush, and extending down to the fasciæ investing the flexor muscles (superficial) of the thumb and fingers, so that at its lower extremity the radial artery was exposed and could have been ligated. This incision was unattended with pain, save where nerves distributed to or passing over the muscular fasciæ were pricked or divided, and even in this case the pain was not at all unbearable. *The incision of the integument was painless*, and the writer would have been unconscious of the injury save from the sensation communicated to his hand holding the knife as it was drawn through the tissues. This observation or experiment was made nearly a year ago. It was applied practically at once to all minor cutting operations. The writer has not incised a felon or bubo since without successfully employing this method for preventing or greatly mitigating pain. Many cases could be given, one will suffice. David Harris, of Vancouver, applied with his second finger of the left hand highly inflamed from a felon, the parts much injured by burrowing of pus. A previous felon had been treated on another finger a few months before, and the requisite incision had given him exquisite pain; the patient therefore apprehended great suffering from any operation on the finger now diseased, and begged for chloroform. However, the finger was soaked for fifteen minutes in warm water containing three per cent. of carbolic acid, dried, and then a brush, dipped in the concentrated acid, drawn over the finger in the course of the intended incisions. These, two in number, were then made, using a thin edged scalpel by a slow sawing motion, allowing only the weight of the knife to make the cut. The patient stated that he had suffered no pain, or not more than would have resulted from handling the parts. The parts healed at once. Sometimes it is necessary after making an incision nearly through the integument, if sensibility becomes apparent, to brush out the wound made with some liquefied acid before extending the incision deeper. This was necessary in a palmar abscess treated by this method without pain. The writer has excised a small tumour partly by this plan. Buboes have been operated on painlessly, and in short, the writer can recommend the plan in any cutting operation where no dissection of the skin is involved, and where all the

pain results from the cutting of the skin. It is hoped that it will be of special service to those who are compelled to operate without an assistant. The writer was thus compelled this summer to remove from his right hand, by an incision of over two inches in length, a large wooden splinter which had been thrust through the palmar fascia, and had lodged under the tendon of the lumbricalis of the index finger. It was done without pain, save where a nerve was divided, The incision healed without scar.

These facts, which the writer believes he is the first to point out, have theoretical relations of great interest which may be discussed in a future paper.—*American Jour. of Med. Sciences, Oct.* 1870, *p.* 573.

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## 43.—THE EXHAUSTING NEEDLE-TROCAR; A MEANS FOR THE DIAGNOSIS AND TREATMENT OF TUMORS AND EFFUSIONS.

By Dr. PROTHEROE SMITH, Physician to the Hospital for Women, &c.

[As we can ourselves testify to the value of Dr. Smith's invention, we give with pleasure the following article on the subject.]

Fine exploring trocars have been long in use, but, from their inability to evacuate viscid fluids, they have failed to accomplish the end intended, and, from their size, to be employed with impunity for puncturing deep structures. To attain these desired objects, I adapted needle-trocars to an exhausting air-pump or glass syringe. The instrument can now be used without danger or difficulty, wherever acupuncture can be employed, for the purpose of diagnosing, evacuating, and treating tumours and effusions of all kinds, whether of joints or cysts containing viscid fluid or otherwise, whether declared by fluctuation or only by the history of the case pointing to this probable issue; in abscess or in abnormal effusion in any of the serous cavities, and in extreme cases of retention of urine, without injury to the bladder. It is also equally efficacious in disengaging excessive collections of flatus, which often prove so distressing in tempanitic distensions of the intestines as to defy all other modes of relief. In the same way, without removing the instrument, remedies can be injected into cysts, abscesses, &c., without any risk of the admission of air. Only a few days since I operated at the Hospital for Women on two cases of ovarian cysts containing respectively ten pints and ten pints and three ounces of viscid fluid; also on a case of ascites, from which I withdrew sixteen pints and a half in less than half-an-hour, with complete relief, yet with no more pain than that of the prick of a pin, and leaving only the mark of the needle on the skin, and therefore requiring no after-treatment.

Three years and a half ago, Messrs. Mayer and Meltzer made me a set of these exploring needle-trocars, with exhausting glass-syringe attached, which I exhibited in August, 1867, at the annual meeting of the British Medical Association in Dublin; at Oxford in 1868; and at Leeds in 1869; I also showed them to several eminent physicians in Paris in 1868, as well as to M. Robert, the instrument-maker, and successor of M. Charrière.

The instrument consists of very fine hollow steel needles gilt, adjusted to a powerful exhausting glass air-syringe; or, as it has since been called in Paris, *aspirateur sous-cutané*, or, "pneumatic aspirator." The syringe, by means of a double-action tap at its distal end, and a spring stop on the piston-rod, forms an air-pump, and in action becomes an exhausted receiver, which enables the operator easily to withdraw or inject fluids. Finding, however, in cases when large quantities of fluid, as in ovarian cysts, psoas abscesses, &c., have to be evacuated, that there was some difficulty in manipulation and great labour in pumping, I suggested the important alterations which I now have the honour to exhibit. This, in place of the exhausting syringe, supplies a glass cylinder of one, two, or three quarts, or more, exhausted by a powerful air-pump, which, by a contrivance in its piston, has a double action of either expiration or inspiration, by which it readily imbibes or expels the evacuated fluid. By means of a long flexible gum-tube attached to the receiver, and terminating, to the extent of three inches, in glass tubing, the cylinder is connected with the exploring-needle. This can now be used with perfect ease and at some distance from the receiver. There being stopcocks at each end of the glass-tube and trocar, as well as at the other end of the elastic tube, the nature of the fluid imbibed is at once detected by the glass portion, which also enables the operator to see the colour and character of the fluid as it rushes past to fill the cylinder. In like manner a small as well as a large quantity of fluid can be easily and rapidly evacuated, without distress to the patient or fatigue to the operator.

In conclusion, I have only to notice the extreme delicacy of the needle-trocars—some almost like bristles—to show the necessity of great caution in their use. They should be introduced —as the needles in acupuncture—by rotating them between the thumb and finger, and holding them as near as possible to their point.

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Since reading the above paper at Newcastle, I have found that the large receiver with double-action cylinder, which I there exhibited, though useful in hospital practice, was too cumbersome for general use. I have therefore substituted a double-action glass-syringe, which, by the ordinary process of pumping,

affords an all-sufficient power, alternately of suction and of expulsion, by which means any amount of fluid may be readily withdrawn. It consists of the exhausting syringe before described; but, in place of a double action tap at its distal extremity, there are two valves placed one on each side. By one of these, when the piston is drawn out, the fluid is imbibed; and by the other, when the piston is depressed, it is as effectually expelled. On the inspiration or suction-valve is a tap, which, when turned off, converts the cylinder into an exhausted receiver as required for the purposes of exploration and diagnosis. When, however, it is necessary only to evacuate fluid deposits and formations, the contents of a distended bladder, or the flatus of tympanitis, it is sufficient to use only the syringe as an ordinary pump. Further, to facilitate its use in withdrawing large quantities of fluid, a pedestal of thick wire, with stirrup-base for the foot, is supplied, which can be easily attached to the syringe, so that, by its help, the operator can steadily hold and work the instrument, when necessary, without the aid of an assistant.

The extreme simplicity of management and portable size of this instrument, as well as the ease with which it may be used in exploration for diagnosis or for the evacuation and injection of fluids, give it a claim to superiority of design and construction which heretofore has never been attained.—*British Medical Journal, Nov.* 26, 1870, *p.* 579.

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## 44.—IN-GROWTH OF THE TOE-NAIL.

By Dr. J. WARING-CURRAN.

The general treatment of ingrowing toe-nail, which is always tedious and frequently troublesome, it is not my intention to review, but as briefly as possible to explain a form of treatment which I have practised for the last three years with uniform success, or, to be accurate, with that amount of success which may be expected resulting where evulsion of the toe-nail is not resorted to, and where, I think, I shall presently show the necessity of performing that simple, though painful, and disagreeable operation, was in several instances overcome. The first four of my patients were cases wherein I had exhausted my experience and book-learning, in order to prevent, if possible, evulsion; but there was little improvement; the cases did not progress as would have been desired; accordingly, I took advantage of a fortunate incident in trying a new method of treatment. At the house of a literary friend, I met an Italian lady, whose high attainments and publications are well known in literary circles. After discussing various topics, in conversa-

tion we entered upon the very remarkable one of "filbert nails," which she told us were cultivated by the ladies of her native town to such a degree that they ignored the wearing of gloves, in order to exhibit the neatness and symmetry of the finger nails. She told me, in order that they may be properly grown, chiropodists, practising the art of nail cultivation, were in the habit of putting their consultees under the following plan of treatment. Out of the centre of the nail they cut a triangular portion—the base at the free extremity of the nail, and the apex at the matrix—so as to encourage the nail to contract from the edges towards the centre; or, in other words, to make the central part of nail grow with greatest prominence. Having three chronic cases of in-growing toe-nails in the district, I bethought me to try the plan of cutting out a triangular central portion, with a very wide base, shaving the edges of the in-growth as thin as expedient with a piece of glass, and tying the separated nail together loosely with a piece of dentist's silk from beneath, and placing between the nail and contiguous soft parts, into which the nail intruded, a piece of thick worsted, coated with mercurial ointment. Where those exquisitively-sensitive granulations existed, I applied some extract of belladonna and resin ointment rubbed together, and adopted the same method in remedying the shape and growth of the nail.

In the course of time the affected nail assumed a better shape, grew out more prominently, and away from the sides, whilst the pain and irritation was overcome by the belladonna application, and eventually cured by the mercurial ointment, and pressure taken off by the better shape assumed by the nail.

As against every method of treatment, which has for its object the cure of in-growing toe-nail without evulsion, it may be said, my plan needs much patience, and requires time and perseverance. The patients were only too glad to have something to do, and to practice it, for there is a great antipathy among them to tearing out the nail by the forceps, even under chloroform. I ignore the ether spray, for I have used it in removing a toe-nail, and should be sorry to depend on its pain-destroying virtues in future.

Nails, thus operated on, acquired a normal shape in six months; whilst in from six weeks to three months, according to the existing severity of mischief in the soft parts surrounding the nail, the toe became healthy. I insisted on the wearing of broad-toed shoes with low heels, that the foot should not be thrown too prominently forward, or the toes be unduly compressed together.

In two of my patients the outer side of toe-nail had overhanging soft parts which appeared healthy on the surface, but

into which the nail was growing; these I shaved off with a bistoury, lifted the nail, took out the triangular portion, and, by stimulating applications, got a flat, healthy surface, which soon skinned over.

It may be readily gathered what I mean to show is that a central portion cut out of the nail will alter the shape of that nail, and, if the disease in the soft parts be attended to, will be found of great practical utility in treating, and altering the shape of, an in-growth of the toe-nail.—*Medical Press and Circular, Nov.* 30, 1870, *p.* 434.

## 45.—ON THE USE OF GLUE BANDAGE.

By Dr. G. H. B. MACLEOD, Professor of Surgery in the University of Glasgow.

I have during the last few months given a more extended trial to the glue bandage, in the form in which the euphonious French have termed the "*bandage gelatino-alcoolisé lucé*," and have had every reason to be pleased with it. It is made by impregnating a roller with a solution of French glue in methylated spirit. When applied a sufficient number of times to the limb to make it firm, it is cut up the front, and finally laced through eyelets put into the cut edges. It is very light and elastic, and can be made of any degree of strength by the number of layers of bandage used. I obtained a tidier and neater, as well as a firmer mould, by putting adhesive plaster within and without the glued portion, a thin layer of cotton wool being next the leg. It is an admirable contrivance for giving support after the removal of splints.

In treating simple fracture of the leg in children, I have obtained the best possible results from employing adhesive plaster, applied in a succession of stripes from the foot upwards (after adjustment, of course), and then two light pasteboard splints, retained at their upper and lower ends by a narrow circular band of plaster. The apparatus is of the simplest description, rapidly applied, cleanly, and permanent.—*Glasgow Medical Journal, Nov.* 1870, *p.* 4.

## ORGANS OF CIRCULATION.

## 46.—TORSION OF ARTERIES AFTER AMPUTATION OF THE LEG.

(Report from cases under the care of Mr. COOPER FORSTER at Guy's Hospital.)

Mr. Cooper Forster has now twisted every vessel, large or small, in all his amputations for four years past, having been

previously as much wedded to acupressure for a considerable period. The result of his experience is remarkable, for we learn that he has not had a single case of secondary hemorrhage during the whole of this period. And the list of vessels twisted includes many arteries of the first size, for he has amputated through the thigh nine times in the last six months, and there are four of his patients with twisted femorals in the wards at this time. We were, therefore, very pleased to have an opportunity of observing closely his *modus operandi* in an amputation of the leg lately. On Tuesday, February 28, a middle-aged man was brought into the theatre with a deformed, useless leg, which it was proposed to remove. The man was a very stout, big fellow, and his left leg, from old disease, was atrophied, the knee flexed and displaced, and the dorsum of the foot occupied by a large gangrenous and exceedingly offensive ulcer. This ulcer had been before healed, but now resisted all treatment, and the man, worn by the distress and inconvenience of the disease, gladly acceded to Mr. Forster's suggestion that he should lose his leg. The only question was whether the limb should be severed above or below the knee, and this point Mr. Forster decided in favour of the minor operation, on discovering, whilst the man was under chloroform, that there was a fair amount of movement in the joint. The operation was performed by two equal flaps, and, the bone being sawn through, the vessels were twisted one after another. The forceps employed were such as are familiar to our readers as torsion-forceps, but the blades were broader and stronger than those we have usually seen, and terminated in broad, well-rounded points. Each vessel was seized and rapidly twisted some six or seven times, the wrist turning with the demi-circuit some twelve or thirteen times, until, in fact, the operator felt something "give;" and the safe completion of the torsion was tested by seeing the twisted bit of tissue pulsating away in the face of the flap. This point is specially insisted on by Mr. Forster. It happened that in this case torsion was particularly difficult. The deformity of the limb made it not easy for the Surgeon to see the wound well, and the larger vessels, as so often happens in leg amputations, retracted considerably, and were caught with much trouble. It was just such a case as has caused many an operator to throw aside the forceps and call for the threads; but Mr. Forster, with more confidence in the method, persevered time after time, until the refractory vessels were secured, and the flaps might be safely sewn up. The impression made upon us by the operation, which, we were assured, presented very unusual difficulties in the application of torsion, was that with such experience as Mr. Forster's before us (and his is not exceptional, for we believe that Mr. Bryant has had

equally favourable results in the same Hospital), and with the manifest advantage of having no threads remaining in the wound—although we confess to being slow to credit the ligature with the many evils attributed to it of late—Surgeons generally would do well to give torsion a genuine trial. We know that it is employed now in many Hospitals, but we believe that if operators saw more of its use they would be glad to adopt so simple and effective a substitute for a process which requires an extra assistant, and is certainly attended by some few evils from which torsion is apparently free.—*Med. Times and Gazette, March* 11, 1871, *p.* 275.

### 47.—HÆMOSTATIC COTTON-WOOL.

Dr. Ehrle describes (*Berliner Klinisch. Wochenschr.*, No. 57, 1870) a hæmostatic cotton wool. The cotton is soaked for an hour in a solution containing 4 per cent. of soda, then washed and dried. It is then dipped one, two, or three times in a dilute solution of perchloride of iron, dried, and pulled apart by the fingers. It is hygroscopic, and is said to be an excellent dressing for wounds.—*Brit. Med. Jour.*, *Jan.* 14, 1871, *p.* 39.

## ALIMENTARY CANAL.

### 48.—ON THE RADICAL CURE OF HERNIA.

By Dr. A. VANS BEST, Late Surgeon, Bengal Army.

Up to the present time, so far as I am aware, seven operations have been suggested for the radical cure of reducible inguinal hernia, all having the same object—the obliteration by plastic material of the aperture of exit.

The very simple operation (even more simple than that of Mr. Redfern Davies) I now recommend requires neither the invagination of the parts nor the use of plugs or buttons, whether of india-rubber or split shot. The steps of the operation are these:—

I use a rather long-handled flat nævus or hemorrhoid needle, well bent (quite a semicircle from shoulder to tip of 1¼ in. diameter), not too wide, and sharpened on both sides from one-third of an inch from the point. A fine hole is drilled for the passage of the ligature, a quarter of an inch from the point. This needle, with a plain dissecting forceps and strong salmon-gut, is all that is required for the operation.

In the first place, chloroform must be fully administered, the hernia reduced, and the thigh adducted and flexed. The finger, as usual, is introduced *quite within the internal ring*, carrying

the integument in front of it up the canal, while the assistant draws the skin of the abdomen firmly over towards the opposite groin. The threaded needle is then passed close to the finger, a small piece of wax having been moulded on its point (instead of a canula); the handle of the needle is raised, and the point pushed through the internal pillar and the abdominal parietes close within the internal ring. The portion of gut on the convex side of the needle is seized by the forceps of the assistant, and the needle, still threaded, withdrawn through all the structures except the temporarily invaginated skin. The finger being carefully maintained *in situ*, the gut on the concave surface of the needle is slightly pulled by the assistant, while that already seized is firmly held; this facilitates the turning of the needle and transfixion of the outer pillar (Poupart's ligament). This being accomplished, the skin of the abdomen is drawn towards the crest of the ilium, and the needle passed through the original aperture, unthreaded, and the finger and it withdrawn. We have, therefore, one scrotal and one abdominal aperture—the latter directly above the aperture of exit of the hernia. Nothing now remains but to tie firmly home the two ends of the salmon-gut, cut it short, and let it drop into the wound. A pad and spica bandage are applied, a dose of opium is given, and the patient kept in bed until the parts are well matted together. The knot of salmon-gut will either become encysted or come away, it matters little which; in either case the approximation of the pillars is certain. It is satisfactory to the operator that the assistant should pass his finger up to the internal ring, when he can distinctly feel it grasped as the ligature is tightened. It is absolutely necessary that the salmon-gut should be soaked in warm water for five minutes before being used, and that long round threads should be selected. The needle should be threaded from the concave side.

I have at present performed this operation only three times—twice with complete success. The unsuccessful case occurred in the European General Hospital, in Calcutta, where the patient, a sailor, leaped over several beds within a week after the operation, and reproduced the hernia.

I would suggest that this operation should be employed when a very large ring exists, so that its space might be diminished, and that the protrusion might thus be brought under command of an ordinary truss.

In the three cases on which I have operated I have had no hemorrhage. One was to all appearance a direct, the others examples of external, inguinal hernia. I give a short summary of my last case.

In consultation with Dr. Pirrie, we agreed that the following was a suitable case for the operation.

R. M——, aged twenty-three, of a nervous and restless temperament, had suffered from reducible right oblique inguinal hernia, of the size of a hen's egg, for three years, accompanied by a small hydrocele. He was exceedingly anxious to have the hernia permanently cured. As he did not pay much attention to the adaptation of his truss, the hernia was gradually increasing in size. At his request I operated (and chose the manner described above) on the 3rd of November, at 3 P.M. He had a grain of opium at once, which was repeated by my orders at 9 P.M., and a little cold beef-tea when desired.

Nov. 4th. Good night; slight tenderness over the wound.—9 P.M.: Passed water; general uneasiness, and some slight tenderness in right iliac region.

5th. Took half an ounce of castor oil; pain towards right iliac region; testicle retracted. Oil operated slightly.

6th. Wound dressed; purulent discharge, redness, and tenderness entirely local.—7 P.M.: Had an attack of colic, which was relieved by camphor and assafœtida pill.

Dec. 17. Not the least impulse on coughing; parts solid. Cured.—*Lancet, March* 4, 1871, *p.* 304.

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## 49.—FEMORAL HERNIA RADICALLY CURED BY OPERATION.

By SAMUEL WOOD, Esq., Shrewsbury.

[The patient, a female, thirty-seven years of age, was admitted into the Salop Infirmary with a strangulated femoral hernia.]

Chloroform having been given, the sac was cut down upon, but not opened; the strictured portion at the neck was divided, and with a little gentle pressure, the contents, consisting of intestine and omentum, returned. It then occurred to me that, if I could detach the sac from the surroundings and invaginate it, and retain it in this inverted position, the raw surfaces would form adhesions, the opening be thus plugged up, and a radical cure be effected. Acting on this idea, I carefully detached the sac, and easily succeeded in pushing it through the openings and so invaginating it. In doing so, I could with my finger detect, through the sides of the sac, the opening of the crural ring and the edge of Poupart's ligament, and feel the end of my finger, covered by the inverted sac, well within the cavity of the abdomen. The sac being secured in this position, the external wound was closed with wire sutures and strips of adhesive plaster, over which compresses of linen were put, and the whole secured by a roller applied in the usual manner. Forty drops of Battley's solution were given. Has vomited

once or twice after the operation, but all the urgent symptoms were relieved. In the evening the bowels were moved.

9th. Doing well; no sickness. Beef-tea, milk, tea.

10th. The dressings were removed, and the edges of the wound found to have united beautifully.

11th. There is a little redness on the edges of the wound, and they appear as if they would separate; a little discharge from one part.

12th. The edges have separated nearly the whole length, and I can see a funnel-shaped depression, with granulating sides, being the inverted sac. She was allowed some wine and a more liberal diet.

From this time the case progressed uninterruptedly; the wound filled up and healed, and a pad was kept on the part to retain it in position until the parts became consolidated. She was discharged cured, and had a truss applied. I saw her some months after, and the part appeared consolidating. She was wearing the truss, and there was no hernial protrusion. I again visited this patient twelve months after the operation; the parts had become quite consolidated, and there was no hernial protrusion.

I am not aware that this plan of permanently closing the opening, and thus radically curing femoral hernia, has ever been done or suggested before. It appears to me so easy and natural a proceeding, and one attended with so little danger, that I trust the publication of this case may induce others to try this plan of operating. It may, however, happen that I am here recording what has long ago been done by others, though I have never found any notice of such an operation.—*Lancet, Jan.* 7, 1871, *p.* 10.

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## 50.—ON THE TREATMENT OF FISTULA.

By J. Cooper Forster, Esq., Surgeon to Guy's Hospital.

*Fistula opening externally.*—B. P., æt. 55, admitted August 24th into Luke Ward; of good general health; works as a smith; no history of syphilis or phthisis. About two years ago an abscess appeared, without any cause that he is aware of, on the left side of the anus. This burst and discharged offensive pus; it has continued to run ever since; no pain; he has had piles, which have got well.

On examination a sinus was found, opening on the left side of the anus, extending upwards about two inches by the side of the rectum. The probe could not readily be passed into the bowel, though the end of it could be felt through the mucous membrane.

25th. A probe director was passed without any difficulty and without pain apparently through an opening into the bowel, just within the sphincter, and the end was brought out through the anus; the probe was then freed with the knife, and the wound dressed with lint dipped in oil.

26th. The lint was removed, and the patient ordered to keep the parts clean; a piece of lint was applied to sop up discharge. He was to remain in bed a few days.

September 3rd. Wound granulating; no pain; discharged convalescent.

*Fistula opening internally.*—J. G., æt. 28, admitted August 29th; railway guard; general health good; no syphilis; no phthisis. Was quite well up to three months ago, when he noticed a swelling which he could not account for near the anus. It was so painful he was obliged to leave off work. Six weeks ago it burst into the gut.

On examination an induration, which was very painful and tender, was found running forward for two inches from the anus along the right side of the raphé of the perineum. On examining the rectum with a speculum an opening was seen on its anterior wall about one inch from the anus.

30th. An incision was made from the rectum into the hardened part, and the wound was dressed as in the former case.

September 8th. Patient in a good deal of pain.

12th. Upon examination to-day a sinus was discovered running forwards, which was laid open and dressed as before.

22nd. Doing well; merely keeping the parts clean, and applying a little bit of rag for the sake of cleanliness.

30th. Patient to get up.

October 12th. Wound filling up; patient very comfortable.

Discharged convalescent.

The first of these cases was one of the most common form of fistula, with an opening externally, and one also within the gut. Though not always easily detected, there is scarcely any doubt to be entertained that upon careful examination an opening—it may be very small—is to be discovered in most cases of fistula of any standing; it may be sometimes at a considerable distance up the gut, but most frequently just within the external sphincter; and there is no doubt, also, that the division of that muscle is all that is required for the perfect cure of the trouble. But no more than the division of the external sphincter is necessary, and this is most easily accomplished in the manner I have directed my dressers now for many years to adopt,—and in all my private cases I have used the same procedure. A grooved probe is passed along the sinus into the gut, and by passing the finger into the rectum, the end of the probe by a little manipulation is pulled down out of the anus; a knife is then run along

the groove in the probe, and thoroughly and perfectly divides the sphincter, and lays the sinus open. I feel satisfied that this is the easiest, simplest, and most efficient plan for dividing the sphincter ani externus, and it is only that muscle that need be divided. I should not dwell upon this excessively simple plan of performing this operation, did I not find the old orthodox recommendation of a blunt-pointed bistoury run along the sinus, the finger introduced into the rectum, and both withdrawn at once so as to divide the sphincter, still recommended in text-books of surgery, and diagrams showing the operation as thus performed still depicted in standard works. In the operation thus performed there is a chance of wounding the operator's finger, and there is a prospect of not thoroughly dividing the sphincter, both of which results are entirely set on one side by the operation I am in the habit of performing.

In the second case there was no opening externally, nor any internally; there was merely a large abscess in close proximity to the rectum, and which very shortly would have established a communication with the gut and the external parts. I therefore thought it advisable to adopt the plan I believe first recommended by the late Sir B. Brodie, and pass the knife, guided by the finger, into the rectum, cut outwards into the abscess through the sphincter at once, thus making one operation answer instead of two. This operation, however, unless performed with Brodie's knife, or unless in skilful hands, is not unlikely to be attended with failure, from want of thorough division of the sphincter. Brodie's knife, I may here observe, is one which is shaped like a bistoury; but the outside edge is sharp instead of the inside, thus facilitating the complete division of all the structures between the gut and the abscess. Of course a well skilled hand would easily do the same with a straight bistoury. To show that the failure is not a fanciful one, I may observe that in this second case the dresser failed, in the first instance, to divide the parts perfectly, and hence the necessity for the second operation.

In dressing these cases after the operation a piece of lint, placed in the wound directly after the division of the sphincter, and kept there until the bowels are open, is, I believe, all that is necessary. There is not the slightest necessity, so far as I know, for what is called dressing the wound daily by stuffing a piece of lint to the bottom of it, so as to prevent adhesion of the edges. No such adhesion is at all likely to take place, and I do not believe it to be possible. I need hardly observe that cleanliness is a necessity where there is so large a discharging surface.

I have not adopted any other mode of procedure for many years than the one I here recommend for the after treatment of

all cases of division of the sphincter. I doubt very much whether the horror with which the public mind is imbued in the matter of "cutting out a fistula" has not something to do with the protracted after attendance, and the unnecessary complication of the operation itself.—*Guy's Hospital Reports, vol.* xvi., 1870-71, *p.* 363.

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## 51.—ON THE TREATMENT OF PILES.

By Dr. G. H. B. MACLEOD, Professor of Surgery in the University of Glasgow.

In several cases of this most troublesome affection treated during the last six months, an operation similar to that employed on the tongue for epithelioma was performed. A long pin was made to transfix the base of the pile (after the patient had extruded it by pressing over hot water), and then, when he was under chloroform, a free incision was made on the skin side, encircling all the tissue to be removed, and the chain of the ecraseur passed over the pin and into the groove so formed. This has usually to be repeated on each side of the anus. In a few seconds the operation is completed, and a comparatively small wound (from the pressure of the chain) remains, which rarely gives out any blood. As compared with the ordinary ligature, this method of removing piles is very superior. It is more quickly effected, the suffering for many days which precedes the separation of the strangulated mass is avoided, there is rarely any retention of urine (and if at all, only for a few hours), and the period of confinement to bed is greatly abridged, as the closing of a granulating wound is all that has to be accomplished. The whole circumference of the anus should never be grasped at once, as such a mode of proceeding is apt to be followed by a temporary closure of the bowel, and hence much annoyance from retained flatulence. I never use any dressing after operation except a wet sponge. No stimulating wash should ever be employed. Cleanliness is all that is required. If the pain is great, morphia may be injected subcutaneously, and in all cases it is well to keep the bowels at rest for two days, and then give a dose of oil. I have never had any trouble, however, when the precaution as to keeping the bowels quiet was neglected, if the wound was only carefully cleaned after stool. The disagreeable feeling of dryness and tension which occasionally follows the operation, can always be relieved by renewing the moisture on the sponge and applying it hot.

The use of the pin and a deep groove on the skin side is very important, as it insures the removal of the whole mass, and keeps the chain to the point where we wish it to act. I have

now employed this mode of operating for a considerable time, and find it everything I could desire.—*Glasgow Medical Journal, Nov.* 1870, *p.* 8.

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## 52.—A CASE WHERE AN ARTIFICIAL PLATE WITH FALSE TEETH WAS ACCIDENTALLY SWALLOWED, DISLODGED, AND ULTIMATELY PASSED BY THE PATIENT.

By HENRY SMITH, Esq., Assistant-Surgeon to King's College Hospital.

The interesting cases of lodgment of artificial teeth by Mr. George Pollock, and the extraordinary instance of the passage of a gold pencil-case through the intestinal canal reported by Sir William Fergusson in the pages of the Lancet, must be familiar to the profession. To these I have now to add a case of much interest, where a very ugly foreign body was got rid of safely, partly by the efforts of surgery and partly by those of nature.

On the night of November 30th, 1870, I was summoned to Mitcham by Dr. Hamilton, who had the wisdom to telegraph the nature of the accident; and I thus went down with suitable instruments. I found a corpulent butcher in great distress, he having six hours previously, by some means or other, allowed his false teeth, with their plate, armed with most formidable hooks on either side to slip down his throat. He at once sent for Dr. Hamilton, who, on passing his finger down into the pharynx, could distinctly feel the foreign body on the right side; but, unfortunately, he had not the requisite instruments, and in his endeavours to dislodge it, the body got out of his reach.

On my arrival, the patient pointed to just above the clavicle on the right side as the spot where the intruder lay. I at once passed a long pair of crane-bill œsophagus forceps, and imagined I could feel the foreign body, but I could not catch it. I tried carefully again and again with other instruments; but, as considerable bleeding ensued, and as there was great distress on the part of the patient, I determined to push the body down into the right stomach, and with that view passed a full-sized œsophagus bougie into the stomach, when the sensation of the presence of the tooth-plate at once ceased.

The patient was ordered to keep perfectly quiet, and to take plenty of gruel porridge and oil. He had no pain at all except for about five hours on the day the foreign body passed away, which event happened nine days after I had pushed it down. The patient has suffered nothing since.

I am aware that it is a dangerous practice to adopt the course I did in this case. I know of two instances where death

followed this plan of treatment, one of them from hemorrhage; but I am not sure that prolonged and forcible attempts to extract such a formidable-looking body would not be attended with as much danger; and there are so many instances on record where such ugly bodies have passed through the intestinal tract with safety, that the surgeon is quite justified in resorting to the expedient I adopted, providing he has first made an effort to extract the substance. The attempts I made were quite sufficient to tell me that I should not succeed in extracting the false teeth; and, indeed, I necessarily put the patient to so much pain and distress, and brought about so much bleeding, that I was only too glad to desist from further attempts. If these attempts do not succeed at once, they are not likely to succeed at all. Remarkable cases have occurred where such foreign bodies have been extracted, and among others, one happened in the practice of Dr. Vine, who succeeded in extracting with a probang a plate, armed with seven false teeth, an inch and three-quarters in length, and which had become engaged in the lower part of the œsophagus.—*Lancet, April* 1, 1871, *p.* 440.

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## 53.—REMOVAL OF A LARGE NASO-PHARYNGEAL POLYPUS WITH WIRE ECRASEUR.

By Dr. THOMAS HAYES, Medical Officer, Shanagolden Dispensary, Ireland.

J. O'Sullivan, æt. fourteen, the son of a labourer, strong and healthy looking, consulted me in the month of May in the present year. "About 2¼ years ago he found his left nostril stopped, he could not breathe through it," it bled occasionally and sometimes considerably; latterly he found a great deal of trouble in breathing, a choking sensation and frequent vomiting; gradually he began to feel drowsy, and for some time before he came to me whenever he rested during the day he dropped off into a *snoring slumber*. His father told me "he was always asleep or going to sleep," which I believe was not a very exaggerated statement, as I found whenever he came to consult me and waited any time before being attended, he usually dropped off into a deep and stertorous sleep, reminding me of the peculiarity which Dickens attributed to Pickwick's fat boy, and which, until then, I regarded as the most extravagant flight of fancy on the part of the great and gifted novelist. He spoke with a very nasal utterance, as one suffering from a severe attack of inflammation of the fauces; his appetite was small, but he had no difficulty in swallowing. He consulted three doctors before coming to me: one said there was nothing

wrong with him; the second was unable to decide what was his complaint; and the third advised him to go to Dublin to get a "chance of his life," as nothing could be done for him at home. He was recommended to me as a *dernier ressort*, by a gentleman of the county who entertained very faint hopes of a successful result for the poor boy. After having heard the history of the case, and having made a slight examination, I came to the conclusion that I had a polypus to deal with, but on carefully looking into the nares no trace of a polypus could be detected; yet the left nasal passage was occluded as no air could be driven through by a forced expiration; but I succeeded in injecting water through the nose into the pharynx. The right nasal passage was partially obstructed; on looking into the mouth the soft palate was seen depressed and convex resting on the tongue. I raised up the palate and tilted up the uvula, and looked into the fauces which presented nothing abnormal: but I did not succeed in getting a glimpse of the overlying tumour, which seemed to have stretched the curtain of the palate; but did not appear beyond its arches. I examined with the finger, and found the tumour pressing on the palate; but my exploration was not sufficiently exhaustive to ascertain its nature; for as soon as my finger extended beyond the arches of the palate, the boy got such a fit of suffocation and vomiting, that I was forced to desist. With a view to try and ascertain the situation and extent of the obstruction, and with a hope—should it prove to be a soft polypus—of detaching it or drawing it into the nasal fossa, I passed an elastic catheter, armed with a strong ligature, through the nasal cavity into the pharynx. There was some difficulty in getting the catheter past the obstruction, but it ultimately glided down by the *left side* of the pharynx, which lead me to believe that the tumour was attached more towards the mesial line than in the vicinity of the turbinated bones or fossa, the usual *habitât* of polypi. I brought the ligature forward through the mouth, attached a small plug of lint, and guided it over the palate as in plugging the posterior nares, and then drew it forward through the anterior nares. No result followed; the polypus was neither displaced nor detached, but I found the throat less sensitive to digital examination.

I again tried to pass my finger *over* the arch of the palate, and I succeeded this time in ascertaining the nature and size of the tumour. I found it round, smooth, and hard, filling up the space between the posterior nares and the back of the pharynx, resting, with the intervention of the palate, on the dorsum of the tongue, occasionally dipping into the opening of the glottis, and producing spasmodic choking fits, which the father stated, so terrified himself, that he was unable to sleep at night for some time before the operation was performed, lest his son should die in one of them.

I determined on removing the tumour with Maw's wire écraseur for nasal polypi. I ordered the instrument, and in the meantime the tumour grew so much, that when he came to me on the 4th of July to have the operation performed, the tumour had grown so large, that it was only necessary to raise the uvula to see a smooth, whitish, rounded body, almost completely blocking up the breathing channel between the nares and larynx. I tried to ascertain its point of attachment by passing my finger about it, but the only thing I could ascertain was, that I could put the loop of wire over it, and so secure its being securely ligatured.

*The Operation.*—I passed a catheter, armed with a ligature, with some difficulty, through the nose into the fauces: I drew one end of the ligature out of the mouth, I withdrew the catheter, and with the ligature pulled the wire looped through the nose, and brought it forward into the mouth: I attached both ends of the wire to their proper place in the écraseur, moving the instrument into the nose. I stood behind the patient, and pressed with my fingers the loop of wire over and in front of the tumour. I kept my fingers on the tumour to prevent the wire slipping off, while my friend, Dr. Madigan, who gave me valuable assistance, turned the handle of the écraseur to tighten the noose and strangulate the tumour. After a little the wire came away, but the tumour was not detached; I then came to the conclusion that the tumour grew from above, and not from the floor of the nose, as one would suppose from the catheter passing outside and behind it; at all events, I felt sure that the loop should pass over *behind* the tumour, I passed the armed catheter again, and this time with two ligatures, to leave one in for an emergency—a very wise precaution as it proved. I pulled the wire through as before, passed the loop behind the tumour, kept the tops of both index fingers steadily on it, to prevent the loop passing over it. When the écraseur was worked, it was very soon evident that something was caught in the loop, at which the wire had some tough work; the boy screamed as it tightened, but on the whole bore it very well without chloroform. After a few more turns of the handle of the écraseur, to my great discomfiture, the wire yielded to the resistance offered by the tumour—a coil of three wires broken by the roughness of a nasal polypus! Who would suppose such a thing? But I must say that the wire did not seem to me to be good, and the tumour was very firm. I then tried the strongest wire of the three sent with the instrument. The supplementary thread left in the nose was now of service (a double one was inserted); the loop of wire was pulled through and adjusted round the neck of the tumour; the écraseur was worked as before. I kept my fingers on the tumour, which I

found was fairly encircled this time, but I now wished to guard against its dropping into the pharynx, and causing suffocation. After a little, the wire came through the foramen of the écraseur. The tumour did not drop down on my fingers immediately; I feared a failure after all—but hold! I find it loose, ready to fall down. I pressed the head forwards, and never removed my fingers from beneath it until it dropped out of the boy's mouth on the floor. I gave a sigh of relief—of joy. What a glorious success due to French ingenuity! I could have elevated Chassaignac to the seventh heavens, so effectual, so decisive was the success obtained by his charming invention. There was no hemorrhage, except a slight oozing after passing the catheter. The patient has felt quite well since the operation. The drowsy symptoms presented in this case were, in my opinion, due to imperfect arterialisation of the blood, and not to any pressure from the nose on the base of the brain. The tumour proved to be a fibrous polypus. It was removed with as clean a cut as if done with the knife.—*Medical Press and Circular, Nov.* 16, 1870, *p*, 391.

## ORGANS OF URINE AND GENERATION.

### 54.—OBSERVATIONS ON THE TREATMENT OF A HUNDRED CASES OF STRICTURE OF THE MALE URETHRA, BY GRADUAL AND IMMEDIATE DILATATION, EXTERNAL AND INTERNAL URETHROTOMY.

By Dr. WILLIAM STOKES, Jun., Surgeon to the Richmond Surgical Hospital, Dublin.

[The hundred cases of stricture, the comparative success of the treatment of which according to the method adopted forms the subject of inquiry in the following paper, were treated as follows.]

First—Gradual dilatation.
Second—External urethrotomy.
Third—Internal urethrotomy.
Fourth—Forced dilatation.

For the treatment of ordinary uncomplicated cases of stricture by gradual dilatation, I must unhesitatingly give the preference to the French "bougies olivaires," and "bougies à boule."

There are some warm advocates for the treatment of stricture by laminaria bougies, but there is one very great objection I have to them, which is, that owing to the dilatation of the laminaria beyond the seat of the stricture, the difficulty in with-

drawing the instrument is, in most cases, very great, and is generally attended with considerable pain.

There can be no doubt that, although the treatment by gradual dilatation is the slowest, it is unquestionably the surest and safest method, and the one which, judiciously carried out in practised hands, is least likely to be followed by relapse.

The operation of external urethrotomy is certainly one of the most difficult, if not the most difficult operation in surgery. This great difficulty arises from many causes, among which may be mentioned, the abnormal condition of the parts to be divided, the impossibility in the great majority of cases of introducing from the wound in the perineum any guide into the bladder, and also the division of the stricture at so great a depth from the surface. Several methods have been devised for performing this operation, among which may be mentioned, the procedures of Hunter, Arnott, Guthrie, Wheelhouse, and Gouley.

In Hunter's operation "a catheter was first introduced as far as it would go, as a director, and all the sinuses were laid open to that catheter, which exposed near an inch in length of that instrument; then the catheter was in fact withdrawn, to expose that part of the urethra which was laid bare. The blood being sponged off, the orifice in the stricture was next searched for, and when found, it was dilated. The catheter was now pushed on to the bladder, although with some difficulty."

Mr. Arnott's operation consisted in cutting down on the point of a sound carried down to the stricture, and then passing a small grooved probe through, and dividing the stricture. Mr. Guthrie recommended opening the urethra behind the stricture, and dividing the contraction by cutting from behind forwards. The full particulars of these procedures will be found in the chapter on "Perineal Section," in Sir H. Thompson's classical work on *Stricture of the Urethra*.

In Mr. Wheelhouse's operation a curved staff, grooved on its convexity, is passed down to the stricture, and the forefinger of the left hand being introduced into the rectum, a scalpel is then entered with its back to the rectum, immediately in front of the anus, and the perineum is opened by cutting upwards and forwards. The urethra is then cut into, not at the point of the instrument, but into the groove of the staff, a quarter of an inch or so above the point. Each lip of the opened urethra is then seized with a forceps, and with these the canal is drawn open. The position of the staff is then altered. It is reversed, the point turned out through the opening, and made use of to draw forwards and steady the urethra. "If the parts are now carefully sponged, I have now," Mr. Wheelhouse observes, "the interior of the canal so fully exposed to *view* that I can steadily follow its upper wall; this I do with a fine

probe director, until I have cut my way completely through the strictures, and have come out into the dilated part of the urethra, which usually lies behind it, or, as sometimes happens, into a second or more strictures; this effected, I am generally able to pass the small director, which I have been using, immediately onwards into the bladder. Having reached the bladder with that, I turn the groove downwards; with a straight probe-pointed bistoury run along it; I make sure of the division of any further obstructing bands, and finally, I completely open up the prostatic urethra by sliding along the groove of the director the little instrument, the 'probe-dilator,' an instrument introduced by the late Mr. Teale, for use in lithotomy, and thus obtain a broad metallic floor, upon which I can, without any danger of failure, introduce a catheter of even the largest size."

Professor Gouley's operation, ingenious though it doubtless is, does not appear applicable to the kind of strictures which hitherto I have treated by external urethrotomy. They were all *truly impermeable strictures*, not allowing the introduction of the finest capillary bougie. In Mr. Gouley's procedure, a fine whalebone capillary bougie is introduced in the first instance. "The next step is to introduce a No. 8 grooved metallic catheter, with a quarter of an inch of its extremity bridged over, so as to convert the groove into a canal, the bridged portion itself being also grooved. Its introduction is accomplished by passing through the canal the fine end of the retained guide, then holding the latter steadily between the thumb and index finger of the left hand, and pushing the catheter-staff gently into the urethra, until its point comes in contact with the face of the stricture. The staff and guide are then kept in position by an assistant, who, at the same time, supports the scrotum. The patient is placed in the lithotomy position, and held by two assistants, or better, by the aid of Pritchard's anklets and wristlets." The surgeon should then make a free median incision, extending from the base of the scrotum to within half an inch of the margin of the anus. Having exposed the urethra, the contrivance of Mr. Avery, of passing a loop of silk through each edge of the incised urethra, and each loop of silk is held by the assistant in charge of the corresponding limb,—"When the urethra is opened and the loops are secured, the catheter is withdrawn a little, so as to bring into view the black guide, then the stricture with about half an inch of the uncontracted canal behind it, is divided." The concluding step of the operation is to pass the catheter-staff, guided by the whalebone bougie, into the bladder; but should it be arrested in its course, the knife must be re-introduced, and the incision extended further back."

The method which was adopted in the following cases differs

in some respects from those I have above alluded to, and will be best understood by a reference to the cases.

*Case* 1.—*Stricture of Urethra, of sixteen years' duration, complicated with Perineal Fistula; External Urethrotomy; Recovery.*—M. H., aged sixty-two, was admitted into the Meath Hospital, under my care, on December 16, 1867, suffering from urethral stricture and perineal fistula, the latter complication having existed for upwards of six years. For some time after his admission, I made numerous but unavailing attempts to introduce an instrument into the bladder. Neither the filiform bougie of Maisonneuve, or the catgut director of Dr. Hutton's railway catheter, or the finest of the self-dilating sea-weed bougies, could possibly be introduced. On consultation with my colleagues, I determined to perform external urethrotomy. Accordingly, on the 18th January, 1868, the patient was brought into the operating theatre, and placed in the lithotomy position. A large silver catheter was introduced into the urethra down to the seat of the stricture, and held firmly in that situation by my friend, Mr. Wharton. I commenced the operation by making an incision with a long straight bistoury along the raphe of the perineum, terminating at a point about three quarters of an inch in front of the anus. The soft parts were divided freely down to the extremity of the silver instrument, which rested against the stricture. I then introduced, and with the nail of my left index finger felt distinctly the contraction beyond the point of the large silver catheter. The stricture I then succeeded in dividing with the point of the straight bistoury, which I used in the first instance for making the external incision. In doing this, which required the most delicate manipulation, I took especial care to keep the handle of the bistoury well depressed. With greater facility than I had at all anticipated, I arrived at the dilated portion of the canal, beyond the seat of the stricture. I then took a full-sized gum-elastic catheter, without any stilette, which I fortunately succeeded in introducing into the bladder, through the wound. The next difficulty I had to encounter was to bring the fore part of the catheter through the anterior portion of the urethra. For accomplishing this, I had recourse to a manœuvre which Mr. Porter suggested, and which was attended with complete success, and I can recommend it most strongly to all surgeons who have to deal with similar cases. It consists in removing the ivory head of the catheter which was introduced through the wound into the bladder in the first instance; secondly, in introducing through the orifice and anterior portion of the canal a conical-shaped bougie, the extremity of which, when brought down to the wound, was fixed firmly into the open end of the gum-elastic catheter. The bougie is then slowly withdrawn through the

anterior portion of the urethra, bringing along with it the gum-elastic catheter. The two instruments are then separated, and the catheter fixed in the urethra in the ordinary manner. This case progressed most favourably. The wound in the perineum slowly but eventually completely closed, and when the patient left hospital, the largest sized catheter could be introduced with facility into the bladder.

*Case* 2.—*Impermeable Urethral Stricture of ten years' duration; External Urethrotomy; Recovery.*—The following case of impermeable urethral stricture illustrates even more fully than the preceding one the great practical value of external urethrotomy.

David E., aged thirty-three, a sailor by occupation, was admitted into the Richmond Surgical Hospital, on December 18, 1869, suffering from a close stricture of the urethra. He stated that about ten years ago, while in London, he contracted gonorrhœa, of which he was never perfectly cured. A gleet was the result, which, in consequence of his irregular mode of living, and frequent exposure to wet and cold weather, became chronic. About a year after he contracted the gonorrhœa, he perceived he was unable to expel the urine in as full a stream as was natural. This state of things continued without his seeking for any surgical advice, until at last he was only able to void his water drop by drop.

About three months previous to his admission to the Richmond Hospital, while helping to unload a cargo of timber with which the ship in which he was employed was freighted, a heavy log of wood struck him on the testicle, which soon after swelled greatly, and difficulty of passing water greatly increased. He then consulted a practitioner in Wales, who recommended him to come to Dublin for surgical treatment. On examining the patient, I determined the existence of a dense cartilaginous stricture, at the membranous portion of the urethra. For upwards of ten days or a fortnight, I made frequent but unavailing attempts to pass the smallest catheter, and I found that not even the introduction of the filiform bougies of M. Maisonneuve was possible. The symptoms of retention of urine at this time became so extreme, and the bladder became so enormously distended, that my clinical assistant fearing a vesical rupture, sent for me, and, after a final attempt to introduce an instrument proved unavailing, I determined to perform a perineal section.

Accordingly, at 4 P.M., on Sunday afternoon of January 2, the patient was brought into the operating theatre, and brought under the influence of chloroform, and I then proceeded to perform external perineal urethrotomy. I may say, in truth, that in consequence of the failing light, the great depth of the perineum, the violent struggles of the patient, over whom

chloroform had but little effect, and the great length of the stricture, through which no instrument could be introduced for a guide, this was by far the most difficult surgical operation it has ever been my lot to perform.

It is unnecessary to go through in detail the steps of the operation I performed, which lasted fully an hour and a half and the various but fruitless efforts I made to introduce an instrument, when I believed the stricture was sufficiently divided. The great length and depth of the stricture and the failing light were doubtless the chief causes of the extreme difficulty I experienced. However, my efforts were at last rewarded, and to my no slight satisfaction, I succeeded finally in introducing a large silver catheter, and drawing off an enormous quantity of fœtid urine. The instrument was then secured in the bladder, and the patient sent back to his bed, and got immediately a powerful anodyne. Five days after the operation, No. 6 gum-elastic catheter was introduced without any difficulty, and the wound in the perineum was found to be rapidly closing. Very little urine succeeded in getting through the perineal wound. Ten days after this the perineal fistula was found to be not larger than the head of a large pin, and this I succeeded in effectually closing by a plastic operation, and on the 28th February the patient left the hospital, the largest instruments being passed without the slightest difficulty, and the perineal fistula being completely closed.

*Internal Urethrotomy.*—There are, as is well known, many ways in which this operation is performed, the majority of which procedures are described more or less fully in most of the works which have been published on the subject of urethral stricture. In none of these published in Great Britain, so far as I am aware, is there any, even a brief description of the method of internal urethrotomy, which I introduced into practice in this country, and which was, I believe, originally devised by M. Maisonneuve, of Paris. Although I have for some years had occasion to perform the operation from time to time, many of my professional friends are still unaware of the steps of the procedure, and I feel, therefore, that no apology is necessary for giving, before the records of my cases, a brief description of the operation.

The first step in the operation is the introduction into the bladder of the very fine and delicate filiform gum-elastic bougie which is not thicker than the double-lengthened cat-gut guide for the "railway catheter" of Dr. Hutton. This being introduced past the stricture into the bladder, the second step of the operation is the attachment of the grooved steel director to the end of the filiform bougie, by means of a screw. When this is firmly fixed, the introduction of the director commences, the

bougie being slowly pushed before it into the bladder, within which it coils. Seldom any difficulty attends this stage of the operation. The third step is the division of the stricture, to effect which the triangular-shaped urethrotome is passed along the groove, situated either on the concavity or convexity of the steel director, in doing which the stricture is divided, but no other part of the urethra is in the slightest degree wounded. This constitutes one of the chief advantages of this operation; that no part of the urethra, either in front of, or behind the stricture, can be divided, an advantage not possessed by any other cutting operation for stricture. This preservation of the urethra from being wounded is effected in the following manner:— The projecting angle of the triangular-shaped urethrotome is blunt, so that its contact with the urethra cannot be productive of any wound or injury whatsoever. The part, however, of the urethrotome between the angle and its extremity is sharp, but does not wound the normal part of the urethra, as this is guided off the cutting portion of the urethrotome by the blunted projecting angle. When, however, the stricture is arrived at, this cannot take place, and this portion of the passage comes necessarily in contact with the cutting edge of the urethrotome which divides it; but no portion of the urethra, either in front of, or behind the stricture, can be wounded. When the division of the stricture is completed, the instruments are at once withdrawn, and a large-sized catheter is introduced. The deviser of the operation has recommended for this purpose catheters made of vulcanized Indian rubber, which, from their great pliability and softness are very grateful to the patient; but I have found that in some cases their softness and pliability renders their introduction a matter of some difficulty. Although it is desirable, after the operation, that an instrument should be introduced, it by no means follows that the introduction of one is absolutely essential to the success of the operation. In one of the cases that occurred in my hospital practice, after the division of the stricture, I could not succeed in getting in an instrument, and the patient was consequently left without one in the urethra, and nothing could have been more gratifying than the result of this case. On the eighth day after the operation the patient, in the presence of a large class, passed water in a full, strong, and continuous stream, after which I passed No. 10 catheter with the greatest ease. A similar circumstance occurred in a case that was operated on in the Richmond Hospital, by Mr. Fleming, and it ultimately did well. It is, however, much better that an instrument should, if possible, be introduced.

I may now proceed to detail, as briefly as possible, the particulars of twelve cases of urethral stricture, which I have operated on by this method.

*Case* 1.—*Stricture of the Urethra of twelve years' standing, from a fall on the Perineum; Internal Urethrotomy; Recovery.*—William G., aged thirty-seven, a painter by trade, was admitted into the Meath Hospital under my care, on October 5, 1864. The patient stated that eighteen years ago he fell on a bar on his perineum, and about five years after that he first observed a difficulty in passing water. The case had previously been treated by gradual dilatation, by sea-weed bougies, with such success that at the time the patient left the hospital, No. 9 catheter could be passed with facility. Since that time, however, the stricture gradually returned, and at the time of his second admission into the hospital, No. 2 catheter was the largest instrument that could be introduced. I then performed the operation in the usual way, and after its completion attempted to introduce a large-sized instrument, but without success. I then left the patient in bed without any instrument being in the bladder. On the 6th day after the operation, I again attempted to introduce an instrument, and succeeded with facility in passing No. 10 catheter. The patient then left the hospital.

*Case* 2.—*Stricture of the Urethra, of eight years' standing; Internal Urethrotomy; Recovery.*—Thomas M., aged fifty, was admitted into the Meath Hospital on September 16, 1864. The stricture, situated in the membranous portion of urethra, was of eight years' standing. The etiology of it was involved in some obscurity, as there was no evidence of the urethra having been injured in any way, or of the patient ever having suffered from gonorrhœa. No. 3 catheter was, with considerable difficulty, introduced on the day of his admission into hospital.

On Sept. 18, I operated in a similar manner as in the first case I have alluded to, and, immediately after the operation, introduced one of the large vulcanized Indian-rubber catheters, the size of which corresponded to No. 10 of the ordinary gum-elastic catheter. This instrument was kept in the bladder for three days, and then withdrawn. Nothing could have been more perfectly satisfactory than the way this case progressed, for, after the operation, there was no hemorrhage, no pain, and no evidence whatever of any constitutional disturbance. On the 25th Sept. I was enabled to pass No. 12 gum-elastic catheter with the greatest ease, and the patient then left the hospital.

*Case* 3.—*Stricture of the Urethra of twelve years' duration; Internal Urethrotomy; Recovery.*—W. B., aged forty-two, was admitted into the Meath Hospital on October 20, 1865. He stated that he had suffered from urethral stricture for upwards of twelve years, and ascribed the disease to repeated attacks of gonorrhœa, which he had had during his youth. For many years previous to his admission into hospital, he was in the habit of

having instruments passed from time to time. Of late considerable difficulty was experienced in doing so, and when he came under my observation in the Meath Hospital, No. 3, catheter was the largest instrument that could be introduced, and that by very delicate manipulation. October 28, I performed internal urethrotomy; in this case dividing the stricture on the under surface of the urethra. After the operation a large conical-shaped gum-elastic catheter was introduced without any difficulty, and kept in the bladder for forty-eight hours. During the convalescence the patient had no rigor or other evidence of constitutional disturbance. On November 5, the patient left the hospital able to pass water in a full and uninterrupted stream.

[Nine other cases are then related.]

In connexion with this operation, the following points are of considerable practical importance for the surgeon to bear in mind:

1. The operation should not be performed unless the grooved metallic director can be introduced with facility. Any forcing of this portion of the instrument into the bladder will, in the great majority of cases, be followed by rigors and other symptoms of constitutional disturbance.

2. The patient's bowels should be cleared by an enema on the morning of the operation.

3. Immediately after the operation, a full opiate with quinine should be given.

4. The catheter introduced after the operation should not be allowed to remain longer than twenty-four hours. If it is, it will probably give rise to irritation, and the formation of an abscess when the stricture is divided.

5. No instrument should be re-introduced for at least three days.

6. The patient should be kept on milk diet for forty-eight hours after the operation, and no stimulants given.

7. For some weeks after the operation, full-sized bougies or catheters, metallic or gum-elastic, should occasionally be introduced. M. Maisonneuve employs metallic bougies of a peculiar form, and there can be no doubt that the introduction of this form of bougie into the bladder can be effected with great facility.

With reference to the "immediate method," or method by rupture, I may mention that I adopted this procedure in eight of the hundred cases alluded to at the commencement of this record, and the results obtained were not such as to make me a very warm advocate for the operation. In three of the cases very violent constitutional symptoms supervened, and in all, with two exceptions, there was rapid recontraction. I cannot, therefore, avoid coming to the conclusion that of the two operations, rupture and internal urethrotomy, the latter operation is

to be certainly preferred, as, in my experince at least, subsequent contraction less frequently occurs. In one operation the stricture portion of the urethra is divided by a single clean incision, and in the other the wound is necessarily a *lacerated* one. In estimating the merits of the two operations of rupture and internal urethrotomy, the following remarks of Sir Henry Thompson cannot but be read with deep interest, coming, as they do, from a surgeon so justly renowned. Speaking of rupture, he observes—"It possesses the merit of simplicity, for provided the first instrument, a guide, is safely passed through the stricture into the bladder, the introduction of the large tube which effects the rupture is only a matter of force. No other operation for stricture is so easily accomplished, or is so unlikely to fail in its performance. To divide a stricture by any method of incision requires much more knowledge, practice, and tact; while it is, I believe, a more perfect proceeding in its results for really hard, old, and contractible strictures than rupture. Without doubt, urethrotomy, whether external or internal (and the latter is mainly referred to here), is infinitely more difficult to perform neatly and completely." . . . . . "I am disposed to think at present that a well performed internal urethrotomy is more enduring in its results than any other operative proceeding."

It is held by some surgeons that no wound in the urethra is made when the bursting or rupture method is employed, but if this were the case, I would ask, where does the hemorrhage come from that is always present to a greater or less extent when this operation is *effectively* performed? That it comes from the forcible laceration of the urethral contraction, there can be little doubt. This constant occurrence affords sufficient evidence of the necessary production of a lacerated wound at the seat of the stricture, and one probably extending to a considerable distance in front of, and behind the situation of the contraction, and I cannot but think that subsequent contraction will take place more slowly in the case where the stricture is incised than in the one where it is forcibly lacerated and torn. The two operations, also, are obviously equally "immediate."

In conclusion, I may state what I believe to be the principles that should guide the surgeon in the treatment of urethral stricture. First, that all cases in which it is possible to introduce a catheter or bougie, should be treated by the method of gradual dilatation, as, in skilful hands, it is the safest, and the most effectual. Secondly, that strictures which are impermeable to catheters should be treated by internal urethrotomy; and thirdly, that strictures that are impermeable to any catheters or bougies should be treated by external urethrotomy. The larger the experience I get of the treatment of urethral strictures,

the more convinced I am that these are the principles that should guide us in treating this disease, and that all surgical practitioners of discretion, refusing to be misled by the dictatorial assumption of some who claim for other methods unvarying success, and universal applicability, will ultimately accept them. —*Dublin Quarterly Journal, Feb.* 1871, *p.* 1.

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## 55.—ON THE REMOVAL OF STONE FRAGMENTS BY SYPHON-SUCTION.

By Professor DITTEL, of Vienna.

The problem in lithotrity has been, to make a fine detritus, and to float it out through a catheter, so that the urethra may not be irritated. Elaborate pumping apparatus have been devised for this end. The latest of these inventions (Robert Collin's) was exhibited to the *Gesellschaft der Aerzte*, by Dr. Mosetig. In order to criticise it, I must first explain the constituent parts:—1. A catheter of large calibre, with a short curve; the vesical end resembles the female branch of the scoop, it has only a posterior wall, the anterior wall of the open slit being closed by an obturator of bone, so that they may not wound the urethra walls in passing. 2. A closed glass cylinder with an opening in its axis before and behind. 3. A suction-pump, the piston attached to a dentated bar, which is moved backward and forward by means of a toothed wheel with a handle attached.

The operation is as follows:—Several ounces of water having been injected through a catheter into the bladder, one opening of the glass cylinder is attached to the catheter, while the tap of the syringe is inserted in the other, and the contents of the bladder are drawn out with some force. With the fluid there comes out detritus, which sinks to the lower side of the cylinder, the water above it filling the cylinder. If now the water be driven back into the bladder from the cylinder, the sand wells up into the bladder, and on the next extraction-movement is withdrawn again, the sand sinks again to the bottom of the cylinder, the water above it remains; and this manœuvre is repeated till the water brings no more sand out with it.

The method is altogether excellent, but in application to the living patient I have encountered difficulties which finally dissatisfied me with the operation. The following are the disadvantages of the procedure:—

1. The apparatus consists throughout of rigid materials, and it is impossible to perform the extraction and injection of the water without giving jolts. These jolts are unpleasant to the patient.

2. The force of suction acts very powerfully, and, as the extraction only occurs mechanically, the operator has no means of perceiving whether he is doing too much or too little. It may easily happen that the vesical mucous membrane presses so forcibly against the opening of the catheter as to become injured. I attempted to avoid shocks by interposing flexible tubes, but these were often closed by atmospheric compression before the fluid could be extracted from the bladder.

3. I will now explain how I was led to the construction of my own apparatus. First, let me mention the method which I apply, in my own consulting-room, for emptying the bladders of patients. In order to keep my sofa from being wetted, I attach to the external end of the catheter a long caoutchouc tube, the free end of which passes into a vessel standing on the floor; in this way the bladder empties itself completely, and not a drop of urine is spilled. In October of this year (1870), a day-labourer had the misfortune to crush the dorsal vertebræ and the corresponding part of the spinal cord by a fall. He was admitted to Ward 42, and we then observed paralysis of the parts below the injury; the urine had to be drawn off with a catheter. When this was done, I found, from percussion of the bladder, that there was still urine left in the bladder, although none more escaped from the catheter. I attached an india-rubber tube, the free end of which passed into a vessel on the ground, whereupon a considerable quantity more of urine escaped before the bladder was emptied. The catheter and tube represented an ordinary syphon, and I had thus practical proof that the urine might be extracted from the bladder, by syphon-apparatus, in the same way as any other fluid from a vessel under similar circumstances. It was indifferent at what rate the urine might escape through the syphon; the only important matter was to determine whether the detritus in the fundus of the bladder could also be extracted by this method. This question was solved by an experiment in my room. I took a drinking glass, filled it with water, put some sand in it, and placed the tumbler on a chest; I now took a silver catheter with large eyes, attached a flexible tube to it, and carried the tube down below the level of the water. On the floor stood another glass vessel, empty. I now sucked the free end of the flexible tube, and then let it hang over the empty glass; immediately, the water flowed over, and also the sand, so far as the point of the catheter passed into the latter. I had now the experimental proof that the syphon would extract sand, and probably also calculus-detritus. As the contact of the catheter-eye with the sand was necessary for the extraction of the latter, I constructed a catheter similar to my *porte-remède*. It is especially short anteriorly; has, moreover, no eye; it is provided

with an obturator, which can be pushed forward on the catheter stem with a screw-movement, and fixed. This closure is necessary to prevent the urethral walls from being injured by the edges of the opening. With this instrument and a long enough syphon-tube, one can extract even coarse detritus with great force. It now remained to apply the apparatus to the living subject.

Among the four patients with stone who entered Ward 26 at the commencement of this semester, there was one to whose case the apparatus was not merely applicable, but was really the only resource left. Herr K., 55 years old, had had cystitis in 1868, and in the winter of 1869 retention of urine occurred, as the climax of difficult urination that had lasted during the twelve months—probably in consequence of swelling of a bilaterally hypertrophied prostate. The first attempts at catheterism, at his own house, were badly managed and with poor results; and not till the beginning of 1870 was complete evacuation of the bladder effected, with a flexible catheter. A complete paralysis of the bladder remained. On the 3rd of May the patient came to Vienna: I found pronounced catarrh of the renal pelves, paralysed bladder, bilateral hypertrophy of prostate, and a phosphatic vesical calculus. As he could not pass a drop of urine himself, I could not reckon on the spontaneous discharge of the detritus, and, after consultation with my colleagues, undertook lateral lithotomy. The phosphatic stone crumbled in the extraction. The wound remained free, but on the 20th of May I had to open an abscess in the left side of the scrotum, and, on the 22nd, symptoms like those of pyæmia developed themselves, only there was no jaundice. The rigors were alarming; during this week they recurred many times every day, once as many as twenty. Temperature reached 104° to 105·5° F. The febrile movement fortunately ended in an attack of cavernitis in the left side of the penis. On June 4 there was notable fluctuation; I made an incision, and for several days a considerable exfoliation of the necrosed erectile tissue went on. The healing of the abscess was very difficult, but was at length finally completed, the patient leaving the hospital on the 31st July, with only a very small sinus left.

On the 12th September, 1870, as I returned from my holiday-trip, I found the sad news that Herr K., had again retention. When I examined him, on the 17th, I found a phosphatic stone of 3 centimetres' diameter. I have already once seen the very rapid formation of a *catarrhal* calculus; but this catarrh, as seen in Herr K., did not appear intense enough to have produced such a stone in six weeks. It seemed much more likely that the stone had at first lain *perdu* in a diverticulum, and afterwards grown out of it. This opinion was made almost certain in the course of a lithotrity.

Under existing circumstances I had no choice, except either to lithotomise again, or to trust to good fortune and apply my stone-sucker to this living patient. I concluded on the latter course. On the 19th, I had the first *séance*; the patient did not suffer much, but in the four first operations I contented myself with the detritus, which I removed with the scoop. On the 15th October I first applied the suction-instrument. In all, I had two *seances* in September, five in October, four in November. On the 13th of November I concluded the operation with a final search, in which no trace of stone could be anywhere detected. In this case the apparatus had fully succeeded. Since that time I have several times had occasion to use the instrument—to discover its faults, and to improve it. In the first place, I saw that the curve of the catheter was too large; the sand is at the fundus, it was necessary to move the catheter round the bladder, and the greater the curve, the more the point rubs the bladder-wall. I therefore made my next catheter with as short a curve as a scoop would have. A second fault was in the edges of the catheter-opening, which it was impossible to have so completely rounded off as not to rub the mucous membrane of the floor of the bladder; this is still more the case when the fluid and the detritus have been extracted, for now the syphon draws the mucous membrane into the opening. This entanglement can be set free by diminishing or stopping the suction, as by raising up the long arm of the syphon.

On these grounds I altogether abandoned the short catheter with the obturator for sensitive patients, and resorted, in such cases, to a closed catheter, with one large eye well forward, very near the point. The point as far as the eye must be filled up, that fragments may not get lodged there and cause injury to the urethra in extraction.

A third defect was this: in order to inject the water into the bladder, one had to take the tube out of the lower vessel. But in the moment when fluid no longer flows out of the bladder, air passes in; it is difficult to prevent this by compression of the tube; indeed, one drives the air in the tube into the bladder. The air may perhaps do no harm, but it is at least superfluous, and the passing in and out of air-bubbles is unpleasant to the patient, and delays the whole process of extraction. I have, therefore, fitted the handle of the catheter with a separate additional piece: this consists of a tube, six centimetres long, into the midst of which a tube, two centimetres in length, opens at right angles. The additional piece has three limbs,—catheter limb, out-flow limb, syringe limb. Where these three tube limbs come together, there is a stomach pump valve-arrangement: in the first position of this the water can only be sent from the syringe to the bladder; in the second position, the water can

only flow from the bladder into the outlet tube. In this way, without removing the tube from the lower vessel, we can prevent the inflow of air, and make the extraction almost continuous, as long as we think proper. There is no doubt that, by such continuous extraction, we spare the patient the irritation from, and impaction of, fragments; and this is clearly a not unimportant argument in favour of the apparatus. But a yet more important merit is that its use eliminates paralysis of the bladder from the list of the contra-indications of lithotrity.

If we do not possess the additional piece and the stomach-pump action, and do not know how to procure them, a common catheter with a good large eye will sufficiently answer our purpose, only we must fill up the point with wax, and fasten a common india-rubber tube to the handle. The wider the calibre of the catheter, the better, because comparatively large fragments are extracted, and the still larger ones probably remain in the bladder for the next crushing.

In chronic catarrh of the bladder, also, the suction-apparatus is to be preferred, because the mucus and pus lying on the floor of the bladder, and acting as the *ferment* of the urine, can in no other way be so completely removed. If one does not thus absolutely cure the catarrh, at least an important symptom is got rid of. But one is often prevented from using instruments, which are technically just the right thing, because there are patients who only bear the use of an instrument with difficulty, and can only bear it once. There are constitutions in which a simple catheterism provokes urethritis. Such patients are not able to bear a suction-process, and one is glad to complete the lithotrity at one operation. But whether such people are better for being left to expel the detritus by their own efforts, I am doubtful.—*Practitioner*, *March* 1871, *p.* 130.

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## 56.—ON DISEASES OF THE BLADDER REQUIRING THE RETENTION OF A CATHETER.

By BARNARD HOLT, Esq., Senior Surgeon to the Westminster Hospital.

[The following case is illustrative of the advantage of the employment of Mr. Barnard Holt's flexible winged catheter (Retrospect, Vol. lxi, p. 189.) The treatment is equally applicable to cases of atony or paralysed conditions of the bladder, associated or not with enlargement of the prostate gland.]

Charles R., aged 52, a traveller, was admitted under my care on August 8th, 1870. He had been frequently the subject of both syphilis and gonorrhœa, and for the last fourteen months

he had experienced great and increasing difficulty in passing his urine, which is now loaded with mucus and pus. The desire to micturate is very frequent, and he cannot expel more than two tablespoonfuls of urine at a time. He has constant pain over the pubes and at the prostate, and also pain in the glans penis after micturition; and a purulent and bloody discharge exudes from the end of the penis. His aspect is careworn and anxious, and he is very much thinner. His nights are restless, and his general health is materially damaged.

An examination per urethram proved the absence of stricture. A large-sized catheter could be readily passed, but it produced great agony as it entered the bladder. The examination per anum proved the prostate enlarged, and it was very painful on pressure. An examination of the urine proved the presence of albumen, mucus, pus, and vesical epithelium. It presented a white filmy aspect, and was highly ammoniacal. About two ounces and a half of urine were removed by the catheter, and no greater quantity of warm water could be injected. A subsequent examination with a sound proved the bladder free from calculus.

Here, then, was a case in which the diagnosis was not difficult, but where the prognosis was doubtful. The examination with the sound proved the absence of calculus in the bladder. The symptoms did not warrant the conclusion that there was stone in the kidney, for in calculus pyelitis there is usually lumbar pain, increased by exertion, and the penis pain precedes micturition, and is always relieved when the bladder is emptied. In calculus vesicæ, or ulceration of the neck of the bladder, the pain is after micturition, and results from the empty bladder grasping the stone, or approximating the ulcerated surfaces. In this instance, the introduction of an instrument produced intense pain as it passed over the neck of the bladder; and when that viscus was emptied, the suffering was greatly increased, but at once relieved by injecting a small quantity of warm water. As mentioned before, the urine was loaded with pus and mucus; and, to assist the diagnosis, it was necessary to ascertain whether this was secreted by the bladder or kidney. All the urine was, therefore, removed by the catheter, and the bladder was washed out with warm water until the water so injected came away clear. A small quantity of urine was now collected in a test tube, as quickly as it was secreted, and, when examined, only a trace of mucus could be detected; and so far, when taken in connexion with the abundant bladder epithelium, tended to prove that the disease was confined to the bladder and prostate. Thus the case was clear: chronic inflammation and temporary contraction of the bladder, with either abscess or ulceration of the vesical aspect of the prostate. There was no reason to

believe, either from the history or the aspect of the patient, that the disease was malignant. With such a diagnosis the prognosis was doubtful; but the treatment was clear: absolute rest in the horizontal position, the retention of a soft and perfectly flexible catheter in the bladder, frequent removal of the fetid urine, injections of warm water after the urine was withdrawn, improvement of the general health by suitable medicines, and the use of opiate suppositories to relieve pain. And this was the outline of treatment adopted; and you have now the opportunity of seeing the result. The patient bears no longer an aspect of suffering, he has gained considerably in weight, his appetite is good, his functions properly performed; his bladder will now hold ten ounces of urine, which is perfectly clear, devoid of pus or mucus, and deprived of the epithelium, which escaped so freely before. There is, however, the anticipated drawback. He cannot empty the bladder without the aid of the catheter, and I fear he never will; but this is of little moment compared with his wretched state on admission, for, being now taught to pass his own catheter, he can relieve the bladder at will.

It is not, however, to cases of enlarged prostate and its consequences that the use of the winged catheter should be confined. A very interesting example of retained urine, the result of atony of the bladder, has lately come under my care.

J. M., aged 65, who, excepting for his bladder ailment, was a moderately healthy man, consulted me for what was supposed to be irritable bladder. He stated that, in consequence of the peculiarity of his business, he had for some years habitually retained his urine for a longer period than was desirable. Two years previously to my seeing him he had retention of urine, and since that period he has been compelled to pass water every hour or hour and half. The quantity ejected is very small, highly ammoniacal, and containing both mucus and pus. His health is now impaired, and for the last twelve months he has constantly worn an india-rubber urinal. An examination of his abdomen revealed a distended bladder which was further confirmed by the introduction of a catheter, and the removal of two pints of fetid urine. I requested his surgeon to empty the bladder twice in the twenty-four hours, and decided when his urethra was less sensitive to introduce the self-retaining catheter, so that he might relieve himself at will. Unfortunately, in passing the catheter, his surgeon lacerated the prostate, which occasioned considerable hemorrhage, and subsequent inflammation of the bladder. Rest, opium, and leeches to the perineum having subdued this, the winged catheter was introduced, and the patient was desired to remove the urine every four hours, and wash out the bladder with tepid water night and morning.

The character of the urine speedily improved; it lost its ammoniacal fetor, and the mucus and pus gradually diminished. His health was gradually regained, and he is now so far well that he can resume his business. The catheter is removed once in ten days.

Most of you will remember the old man with paralysis of the bladder associated with hemiplegia, who wore one of the winged catheters for a period of five months. In his case the bladder was so small that it would not hold more than six or seven ounces of urine; so that it was necessary (previous to the winged catheter being worn) to draw off the urine six times in the twenty-four hours. Being incapable, in consequence of his paralysis, of passing a catheter for himself, he became a great tax on the house-surgeon; but after the winged catheter was inserted, no further trouble was occasioned—the nurse could remove the peg and relieve him without difficulty.

I could multiply these cases both from my hospital and private practice, were it necessary to do so: but I feel that the examples I have related to you are sufficient to show you that in cases of enlarged prostate, and atony or paralysis of the bladder, the winged catheter is of essential service; and in most cases it can be worn with but trifling inconvenience.

I will now relate an interesting case of numerous fistulous openings in the perineum and scrotum that was speedily cured by the retention of the catheter.

An officer in the Indian army, who had long suffered from stricture of the urethra, consulted me in consequence of the serious inconvenience occasioned by the urine escaping from numerous channels in the perineum and scrotum. He had suffered from extravasation of urine, and from abscess; and he was now compelled to sit over a large pan whenever he required to pass his urine. There were two fistulous apertures in the perineum, one in the scrotum, and a fourth in the buttock.

At my first examination I experienced great difficulty in passing a No. 1 catheter. There was a tight stricture in the spongy portion of the urethra, which grasped the instrument with great tenacity. On my next seeing him I passed a No. 1 gum-elastic catheter, which was retained, and in two days I was enabled to introduce the dilator, split the stricture, and pass a No. 9 winged elastic catheter. He suffered very little incovenience from the operation, and he wore the catheter with ease. The urine now escaped entirely through the catheter, and he could remove it at will. After the lapse of a week it was withdrawn, and found to be slightly coated with phosphates. This was washed off, and the same instrument again inserted. To prevent the redeposit of phosphates he was directed to inject a weak solution of hydrochloric acid twice a day; and when the catheter

was again removed it was free from any deposit. This gentleman continued to wear the catheter for a period of four months, when the fistulous openings had entirely healed. The instrument was then removed, and the urine flowed in a good stream. He was taught to pass his own bougie, and has returned to India perfectly well.

I have now related to you four cases as examples of different forms of disease in which the winged india-rubber catheter may be worn with advantage, and I wish particularly to impress upon you the fact, that where a catheter *can be borne* the india-rubber one gives the greatest comfort, and secures to the patient the command of the bladder without the chances of its escaping, and without the necessity of its being fixed by tapes or any other contrivance.

I have used the expression "where it can be borne," because there are many cases where the retention of any instrument excites so much irritation that the patient cannot bear it, and in private I have found this more frequently than in hospital practice. Private patients, as a general rule, will not persevere where pain is occasioned, and they will not consent to remain in bed until the urethra and bladder become accustomed to an instrument. In hospital cases the patients are under more control, and you will often succeed with them when you cannot do so in private. On the other hand, there are a certain number of sensitive persons who cannot bear the retention of a catheter under any circumstances, consequently you must not consider this method of treatment as applicable to all. Where any other catheter can be retained there, the india-rubber one is infinitely to be preferred, but where the patient is specially sensitive, some other plan must be adopted.

To estimate the value of this kind of treatment you must inquire into the pathological changes that take place in the genito-urinary apparatus. Retained urine not only produces enlargement of the bladder and distension of the ureters and pelvis of the kidney, but disease of the kidney itself, and it is only when the bladder is enormously distended with urine that it can be acted upon by the pressure of the abdominal muscles, the contraction of which squeezes out a small quantity of urine, which gives relief for the time. But in cases where the bladder is hypertrophied and contracted, the patient suffers from absolute retention; the bladder has no power to contract, and no muscular force of the abdominal walls can assist it; and when we recollect that in addition to this, retained urine produces chronic inflammation of the bladder, resulting in not only decomposition of the urine, but a large secretion of mucus and pus, with shedding of bladder epithelium, we cannot be surprised at the fatality of such cases. The patient in very many instances

not only endures extreme suffering, but he is continually poisoned by the absorption of the perverted urine, and gradually and surely dies.

I have hitherto only spoken of the mechanical means by which good can be effected; but, although it must be admitted that, in the main, mechanical means must be resorted to, yet great assistance is frequently obtained by the judicious administration of certain drugs. Copaiba is frequently very serviceable. Hydrochloric acid and opium may be prescribed with advantage. Quinine, where the health is failing, is especially valuable. But, whatever adjunct you may choose, nothing will supersede the removal of the urine and washing out the bladder with tepid water. I have occasionally used weak hydrochloric acid, or highly diluted carbolic acid; I cannot, however, say that I have ever seen any decided advantage from either, and I believe warm water is better than anything else.

The catheter is made of india-rubber, with wings to prevent its escape from the bladder, and is similar to that of which there was an engraving in the Lancet some months since. It has the advantage of being self-retaining, and is indestructible; and it may be worn for any time without removal, provided it remains free from phosphatic deposit. To determine the special tendency to this, I always advise that the catheter should be removed at intervals, at first of four or five days, the intervals being gradually increased in accordance with the tendency to deposit; for while in some cases it may be worn for a month without any accumulation on its end, in another phosphates may be detected after the expiration of a few days. This rule should not under any circumstances be departed from.

In order to prevent this accumulation, I have latterly used the catheter with the addition of a highly polished silver cap, which not only facilitates its introduction, but certainly prevents phosphatic deposit in anything like the time that it will become affixed to the india-rubber. Mr. Baker is now making these caps, which will before long be ready for use.—*Lancet, Dec.* 17, 1870, *p.* 843.

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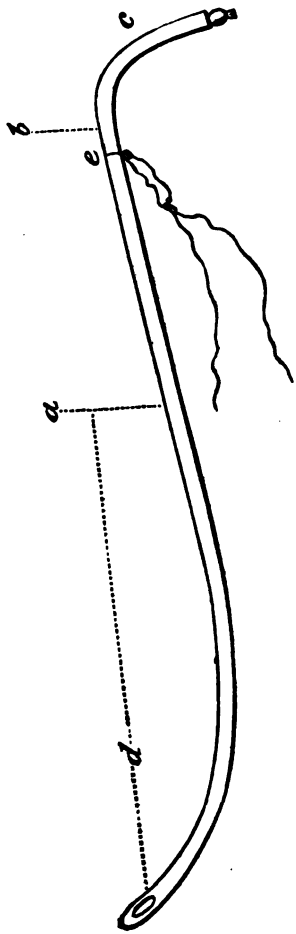
## 57.—ON AN IMPROVED VULCANISED INDIA-RUBBER CATHETER, FOR RETAINING WITHIN THE BLADDER.

By Sir HENRY THOMPSON, Surgeon-Extraordinary to H.M. the King of the Belgians; Professor of Clinical Surgery and Surgeon to University College Hospital.

For many years I have used the vulcanised india-rubber catheter for tying in the urethra and bladder, and have found it, as many others have done, extremely valuable from its perfect flexibility, and the consequent small liability to excite irri-

tation which its presence produces. In most cases it is easily kept in place by a loop of soft twine, loosely tied round the penis, just behind the glans; but in a few instances this very quality of flexibility, which is so advantageous, permits the escape of the instrument from the canal, and difficulty is experienced in efficiently retaining it there, Hence Mr. Holt's suggestion of "wings" to the end of the instrument which lies within the bladder. Efficient as these may be when *in situ*, although they unquestionably sometimes occasion irritation, an instrument so furnished does by no means always pass easily, and usually causes more uneasiness, both in the act of passing and afterwards, than a catheter not furnished with such appendages.




Improved vulcanised india-rubber catheter.—*a* to *b* shows the length of the German-silver tube which at this part lines the tube and stiffens it. *c*. Anterior portion of catheter, which remains flexible, and conducts away the urine; a peg is fitted into it. *d*. Posterior portion of catheter, which remains flexible, and rests in the urethra and bladder. *e*. A piece of stout silk, the free ends of which, lightly tied behind the glans penis, secure the catheter in its place.

The problem, then, is to efficiently, maintain the soft vulcanised catheter in place without making additions of this kind. This I have accomplished, I believe completely and successfully in the manner following:—Into an ordinary vulcanised india-rubber catheter, say about the size of 8, 9, or 10, or more, according to the requirements of the case, a thin German-silver tube, about four or five inches long, is introduced by the maker, so that the last six inches of the catheter remain as flexible as

ever, also about two inches of the anterior part, to form a spout or conductor outwards for the urine. This being done, the calibre of the interior is still nearly uniform,—the thinness of the metal tube and the elasticity of the india-rubber being so accommodated to each other as to accomplish this object. This description has been practically realised for me by Messrs. Weiss, the well-known instrument makers. (See figure.) The instrument is furnished with a silk cord to fasten it, which, owing to the metal-tube, cannot diminish the choke or diameter.

The advantages of this catheter are several:—

First, not only is it not more difficult to pass, but less so than the original instrument without wings, since the metal tube affords a handle which almost invariably carries the flexible part beyond it easily into the bladder.

Secondly, when the loop of silk with which the catheter is provided is loosely tied behind the glans, it is next to impossible for the instrument to escape; the want of flexibility in the silvered part securing this. In special cases in which it is desired, the silver may be shortened to two or three inches with perfect security.

Lastly, since no appendage exists in that part of the catheter which is within the bladder, the liability to retain any phosphatic deposit is reduced to a minimum, and no special attention to remove such foreign matter is always necessary. In one instance I kept it in six weeks without the slightest incrustation occurring. It is, however, as a rule prudent to remove the catheter every few days to observe if any takes place, and this is a matter of no consequence, as no difficulty or pain arises from its reintroduction.—*Lancet, Feb.* 11, 1871, *p*, 185.

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## 58.—ON THE TREATMENT OF INCONTINENCE OF URINE.

By Dr. WILLIAM THOMSON, Peterborough.

Six months ago a patient informed me that her daughter, aged twelve, suffered from this complaint, but supposed medicine would not do her any good, as she had suffered from it for two years. She looked healthy, not at all anæmic, appetite good; in fact, enjoyed perfect health, with the exception of the frequent desire to pass water, and the inability to retain it during sleep. As I was anxious to show my patient that medicine would do good, I thought chloral hydrate likely to have the desired effect, at least in checking the nocturnal symptoms. I ordered her fifteen grains every night on going to bed, to fast from 7 o'clock till next morning, and to eschew beer and spirits. I did not see her for two days; when her mother informed me she was cured. The very first night she

rested well, and did not get up once, instead of four or five times as formerly, besides wetting the bed nightly. She still had the desire to go frequently in the day time. I ordered her, therefore, to continue the medicine for a fortnight, decreasing the dose to ten grains, which she then discontinued entirely, and was perfectly well when I saw her last week.

I felt delighted with the success of the chloral in this case, but waited anxiously for further trial, which, I am happy to say, came soon afterwards. I was applied to for a boy aged thirteen, who had nocturnal incontinence since he was six years old; but not the day symptoms. I gave him fifteen grains of chloral at bedtime. He slept well till five in the morning without his usual misfortune. At the end of a week I advised him to have some more medicine; but he said he thought he did not require it, as last night he omitted to take his dose, and all was well this morning. Continued the medicine a week longer —i.e., three weeks ago. Since then he has remained quite well.

Now, here is a remedy most simple in its action, without any of the unpleasant effects of belladonna on the vision, &c. Where the case is merely one of habit, as both the above, we could hardly expect a more simple and efficient remedy than chloral, which, by its ensuring the bladder and sphincter vesicæ a quiet night's rest, enables them in a few days to regain their normal tone, and the patient his wonted vigour, so that he may retire to rest without his former dread of a disturbed night, or of awakening in the morning to find he is a defaulter to the laws of cleanliness and health.—*Lancet, Nov.* 19, 1870, *p.* 703.

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## 59.—ON THE FUNCTION OF THE PROSTATE GLAND.

By Dr. KRAUS, Editor of the *Vienna Medical Times.*

I believe that I have discovered some essential points relating to the function of the prostate; but I will at present only make some preliminary statements that I think will be found to deserve attention.

1. The seminal fluid, as long as it remains within the testes, vesicles, and other seminal passages, is colourless and scentless, being in appearance exactly like fresh honey while deposited in the comb; and in its reactions it is neutral.

2. Only when it has quitted the passages and arrived in the urethra does it acquire its white colour and its peculiar faint smell.

3. During its passage through the prostatic portion of the urethra, the prostate empties out its fluid, colours the semen white, and confers upon it the faculty of coagulating when

exposed to the air (alkaline reaction). Semen taken from the seminal vesicles does not coagulate, but remains clear, colourless, and scentless.

4. The spermatozoa, in the absence of the prostatic fluid, cannot live in the mucous membrane of the uterus of mammalia; but with its aid they may live for a long time in the uterine mucus, often more than thirty-six hours.

I have conducted these experiments with the greatest care, and recommend their repetition, in order that the truth of my positions may be ascertained. From the above, the conclusion may be drawn that the prostatic fluid exercises an unlimited influence on the viability of the spermatozoa, sustaining it when endangered by the mucus secreted by the mucous membrane of the uterus.

This is undoubtedly the case with those species of animals, which possess a prostate, and I intend next to extend my investigations to those species which are destitute of this.—*Medical Times and Gazette, Feb.* 11, 1871, *p.* 170.

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## 60.—CONTRIBUTIONS TO THE PATHOLOGY OF THE PROSTATE GLAND.

### By Dr. KRAUS.

It may now be laid down as a rule, admitting of but few exceptions, that all diseases of the prostate take their origin in catarrh of the urethra or bladder. Also, the diseases of the seminiferous organs must, without exception, be referred to blenorrhagia of the urethra; so that, in future, what have been considered as sympathetic affections of the vesiculæ, the vasa deferentia, and the epididymis must be regarded as an extension of the blenorrhagic inflammation of the urethra. The fifteen or sixteen of the prostatic ducts, too, which open near the caput gallinaginis are affected with this catarrh when the blenorrhagia extends to the prostatic portion of the urethra. In consequence of the entrance of large quantities of the catarrhal secretion, the gland becomes greatly swollen and enlarged. It would, indeed, be remarkable if these ducts, the office of which it is to discharge the prostatic fluid, should not participate in the general catarrhal process. The enlargement of the prostate about the twenty-fifth or twenty-sixth day of the blenorrhagia is solely attributable to the blenorrhagic affection of its ducts. The considerable muscular elements which the prostate contains is kept, by means of the catarrhal irritation, in a state of constant contraction, inducing increased excretion from its ducts, and contributing to that large discharge from the urethra which is observed when the prostatic portion of this canal participates

in the blenorrhagic process. I have exhibited the truth of this statement by injecting blenorrhagic matter into the prostatic portions of the urethras of five dogs, and finding, on making a section of the prostate twenty-four days afterwards, that it was loaded with secretion. Of the blenorrhagic inflammation of the prostatic ducts, and consequent enlargement of the prostate, there can be, then, no doubt. Even in catarrhal affection of the urethra, which to the observer does not seem to involve the prostatic portion, the prostate may still be swollen. This frequently occurs, too, in chronic catarrh of the bladder, when some of the secretion expelled from the bladder, and detained in the prostatic portion of the urethra, excites inflammatory action in the prostatic ducts. I possess two preparations in which the dendritic ramifications of indurated catarrhal secretion, like that observed in the bronchi in croup, are seen within the canals of the prostate. These preparations confirm the observations that I had already made, that in catarrh of the bladder, owing to the loss of tone in the organ, the secretion becomes arrested in the prostatic portion of the urethra, and, subjected to pressure there, is forced into the ducts. The cavity of the caput gallinaginis also becomes filled with the secretion, and from thence the catarrhal inflammation spreads along the ejaculatory ducts to the vesiculæ and epididymis.

I have, in a former work, shown, by exact observation, that in several cases the copulative power becomes lost through the agglutination or entire adhesion of the ejaculatory ducts. These observations have been corroborated during the present investigations on the condition of the prostate. In eight of the preparations of catarrhal affections of the bladder, the ducts of the prostate were found loaded and impermeable, and in two the vasa deferentia were adherent. The so-called bloody semen is a fiction, for neither in the vesiculæ or the other seminal passages have I ever met with it. It is only rendered bloody when, in blenorrhagia or vesical catarrh, the semen is forcibly expelled through the adherent ducts. The patients, especially young persons, are enabled to indicate the point of rupture if they indulge in coitus during the blenorrhagia. There are two possible cases—either the advancing column of semen has power enough to burst through the adhesion of the ducts, and then bloody semen results; or, when it is deficient in this, the patient is aware of a sense of regurgitation, and the semen, remaining awhile in the passages, is soon forced backwards. These conditions may be observed by those who examine the subject with the necessary care.

The tumefaction of the prostate may be only a temporary symptom, disappearing with the blenorrhagia or the vesical catarrh. When, however, there is persistent obstruction or

adhesion of its ducts, as my former investigations have shown that the discharge of the prostatic fluid is an indispensable condition of the exercise of the generative faculty, we have to consider carefully the condition of the prostate and its ducts as well as that of the seminal passages. For having demonstrated the blenorrhagic affection of the prostatic ducts we claim entire priority, notwithstanding any accidental recognition of similar results by former authors. We have in former investigations shown that the prostate is to be regarded as no mere accessory organ, but as a *sine quâ non* of the entire generative apparatus, complete generation being impossible in an abnormal condition of this organ. The prostatic fluid must, in its chemical and other conditions, be in a pure state in order that generation may be accomplished. But in blenorrhagia which has extended to its ducts, this function becomes impeded, for the quality of the prostatic secretion often undergoes alteration for a long period; so that persons affected with blenorrhagia of the prostatic ducts and their secreting elements are incapable of generation because the prostatic fluid is in an abnormal condition.

The diagnosis of blenorrhagia of the prostatic ducts is very difficult as regards specially characteristic symptoms, although the participation of the prostate in the diseased conditions of the urethra and bladder can be made out in a somewhat undefined manner. Neither any considerable amount of pain nor enlargement of the lobes of the organ, except of the posterior in an advanced stage of the affection, furnish indications of the blenorrhagic affection of the organ. From the twentieth to the twenty-fifth day of its duration the attentive observer may, however, have an opportunity of observing a change in the colour and consistence of the secretion. The whitish mucus may all at once assume a greenish-yellow colour, and be greatly augmented by pressure on the root of the penis. A prickling sensation in the rectum indicates an abnormal condition of the posterior lobe, without determining the nature of this. An important sign that the prostatic portion of the urethra is affected in vesical catarrh is the difficulty with which a catheter is passed into the bladder. This, which otherwise can be passed in with ease, also now induces severe pain while traversing the prostatic portion, even when the blenorrhagia is in its chronic stage. The lancinating pains shooting down to the testes, and the pains in general which irradiate more or less over the whole genito-urinary apparatus in its inflamed condition, have nothing special about them in this affection. In the dead body, when the prostatic ducts have participated in the blenorrhagia, the whole prostatic portion of the urethra is found intensely injected, and on pressure being made upon the prostate, true

blenorrhagic secretion is discharged from the ducts. It is of great interest, too, to note that even when the blenorrhagia has entirely ceased in the urethra, it may still persist in the prostate. We have two beautiful preparations, in which, while the urethra is entirely free from blenorrhagia, the ducts of the prostate, and even the acini, as shown by the microscope, are loaded with blenorrhagic secretion. The secretion was so abundant that on pressure being made it issued abundantly. We had occasion to observe this in a remarkable manner in a patient dying of typhus. Along the entire urethra, no trace of blenorrhagia of recent origin was observable; but the trigonum, the caput gallinaginis, and inner orifice of the urethra were in a condition characteristic of this affection of the prostate—viz., hyperæmic, with small erosions, and swollen.

Some authors who do not pay sufficient attention to the anatomical conditions, and seem not to be aware that the prostate is both in front and behind encapsulated in a very tense membrane, describe abscesses which, forming on the surface of the gland, penetrate into its cavity. In the innumerable cases of diseases of the prostate and bladder which we have hitherto examined, we have never met with such abscesses, except where an unskilful operator has perforated the gland with his catheter. The muscular tissue is so prevalent in its texture, that the formation of abscess in its substance must be a very rare occurrence, seeing how frequent are abscesses of muscle. Isolated by means of ligaments and tendons and its own strong fibrous covering, the prostate does not participate in the diseased condition of surrounding organs, and, with the exception of the parotid gland, there is no organ which so well preserves such independent condition.

On slitting up the prostatic ducts affected by blenorrhagia, they are found to be dilated to three or four times their natural size. In their normal condition there are only five or six of the sixteen ducts that will admit even a fine bristle, the calibre of the others only being ascertainable by the microscope; and their dilatation is entirely due to the presence of so large a quantity of secretion during the blenorrhagia of the prostatic portion. Not only do they become larger, but also longer, some of the ducts, which in the normal condition are scarcely a line in length, becoming two or two and a half lines long. A slate-coloured coloration surrounding the mouths of the ducts is characteristic of their condition, and not only are the ducts pigmented in a similar manner, but they exhibited a slate-coloured deposit, which, under the microscope, is shown to contain crystals of triple phosphate. This coloration is to be distinguished from a post-mortem appearance, and from one which may be artificially produced by passing the knife over the mucous mem-

brane. In the pathological condition, there is a deep blue colour around the mouths of the ducts, disposed in circular stripes, while the post-mortem appearance is diffused without any streaking. In the pathological process there is also a slate-coloured deposit, which is never absent in the *trigonum Lieutaudi*.

After chronic catarrh of the bladder, the careful examination of the prostatic portion of the urethra should never be omitted. Small erosions at the inner end of the urethra should serve as an indication that the prostate may be involved, and lead to its examination. This will, sooner or later, lead to the pathology of the prostate entering upon a new phase. As we have already shown, too, the catarrhal secretion expelled from the bladder may, by the continous pressure it exerts on the prostatic portion of the urethra, give rise to dilatation of the prostatic ducts and the forcing of the discharge into these, in this way leading to the propagation of the catarrhal condition of the bladder to the prostate. In such cases, we find exactly the same secretion, which is accumulated at the base of the bladder; and it is the near resemblance which this often has to pus which has led some authors to mistake it for suppuration of the prostate. The great difficulty which exists in distinguishing this secretion from pus, leads to the suspicion that suppuration of the prostate is commencing; but if all the cases presenting secretion having this appearance were actually of the dangerous character attributed to them, even by the most recent pathologists, we should have to pronounce a fatal prognosis in one half of the cases of vesical catarrh. In most cases of this affection, after a time, pus is found in the secretion without the condition of the patient undergoing any essential change. This presents us with an unwelcome example of how little the histological investigations concerning the innocent or malignant nature of cells have advanced our science: for nowhere more than with respect to the secretions of the diseased urethra can the worthlessness of the hitherto current theories on the mischievous effects of pus be better shown.

The posterior portions of the prostate, on account of their more intimate connexion with the urethra, participate more readily in its affections, and during the inflammatory stage of prostatic blenorrhagia, these lobes may undergo a considerable increase in size without this being perceptible during life. After death, one or other of the lobes may be seen to have undergone very considerable increase without producing any stricture in the rectum, this being rather dislocated than obstructed by its increase. The examination of the prostate frequently gives rise to deception, for the gland must have increased three or four times in size before it can be reached by the finger, and a lesser amount of enlargement cannot there-

fore be diagnosticated. An increase of the gland, such as can be ascertained per anum, can only arise from a degeneration of its entire posterior lobe; and at present we have only to do with the blenorrhagic affections of the prostate. The tensity of its capsule prevents any very great enlargement, but sometimes a portion of the glandular substances perforates this, and projects in the form of a process into the rectum. The same condition may ensue from a mechanical injury done to the gland while passing a catheter.

Strictures of the urethra from enlargement of the prostate are of extreme rarity, nothwithstanding that this is regarded as a frequent cause. The urethra has so large a play between the corpora cavernosa, and can exert much locomotion before being interfered with by an enlargement of the prostate.—*Medical Times and Gazette, March* 11, 1871, *p.* 271.

## AFFECTIONS OF THE EYE AND EAR.

### 61.—ON THE OPHTHALMOSCOPIC EXAMINATION OF HEALTHY EYES.

By J. Soelberg Wells, Esq., Ophthalmic Surgeon to King's College Hospital.

In examining an eye with the ophthalmoscope, it is generally necessary to have a dark room* and a bright, steadily-burning oil or gas lamp. The best gas lamp is that employed at Moorfields, which has an Argand porcelain burner perforated by small apertures, and closed underneath by fine wire gauze, which regulates the draught and steadies the flame. It is attached to a movable arm permitting of universal movement in all directions. The burner is to be covered only by a chimney, without a globe. In place of gas, an oil or paraffin lamp may be employed.

In the selection of a portable monocular ophthalmoscope, I think our choice lies between the instruments of Liebreich and Coccius. The former consists of a concave metal mirror of eight inches' focal length, having a small central aperture; behind the mirror is a clip to hold a convex or concave lens. In Coccius's instrument the mirror is plane, and there is a biconvex lateral lens of five or seven inches focus held in a clip and mounted on a jointed bracket. It is to be used in the following manner:—The lateral collecting lens is to be turned towards the flame, which should be somewhat more than twice the focal length of the lens from the observer. The mirror is then to be

* Dr. Lionel Beale has lately invented an ingenious ophthalmoscope which may be used by daylight.

set somewhat slantingly to the lateral lens and the eye of the patient. If the mirror is properly adjusted, we shall obtain, if we throw the reflection on the palm of our hand or the cheek of the patient, a bright circle of light, with a small dark central spot, which corresponds to the opening in the speculum. Liebreich's is an excellent instrument, and, as it is somewhat easier to use, is more generally employed; but I rather prefer that of Coccius, chiefly for the following reasons: the lateral lens enables us to alter the focal length of the mirror and the intensity of the illumination. Moreover, the reflection is much less, which is of much importance when we examine eyes whose pupils are very small.

In making an ophthalmoscopic examination, the surgeon seats himself directly in front of the patient, at a distance of from 14 in. to 18 in. He then places the mirror, held in his right hand, close to his eye, and turns it slightly towards the flame; so that the rays from the latter may be reflected into the eye of the patient, whose pupil will then be brightly illuminated, and present a brilliant red glow. The surgeon may now avail himself of one of two methods of examining the details of the background of the eye—viz., the direct or the indirect mode of examination. With the former he obtains an erect image of the fundus; whilst with the indirect the image is reversed. He should be equally well versed in both modes of investigation, to the explanation of which we must now pass on.

*The examination of the actual inverted image, or indirect method.*—The pupil having been well illuminated with the mirror, the patient should be told to look somewhat to the left (if his right eye is under examination), at the surgeon's right ear, and *vice versâ*. In this way the optic disc, which is situated to the inner side of the optic axis, is brought directly opposite the surgeon's eye. The latter will now at once notice that the reflex is no longer bright red, but of a whitish-yellow tint. When this change of colour is obtained, the surgeon should take the rim of a biconvex lens (termed the object-lens), of 2 in. or 2½ in. focus, between the forefinger and thumb of his left hand, and hold it 2 in. or 2½ in. from the eye under observation, his ring-finger being placed against the upper edge of the orbit, so as to steady the hand; the upper lid may, if necessary, be at the same time lifted with the tip of this or the little finger. With a little practice and perseverance the surgeon will soon succeed in obtaining an inverted aërial and enlarged image of the optic disc, which will be situated between the observer and the object-lens, and at about the focal distance of the latter. This varies, however, with the refraction of the patient's eye. In order to magnify the image still more, an object-lens, of 3 in. or 4 in.

focus, may be employed, or a convex ocular lens of 8 in. or 10 in. focus, may be placed in the clip behind the mirror.

*The examination of the virtual erect image* (*direct method*).—In this the mirror alone is used without the object-lens; and the surgeon must approach his eye very close to that of the patient, if he wishes to distinguish the details of the fundus, of which he gains a much larger image. Hence this mode of investigation is very useful in solving doubts (which may exist in the indirect method) as to the exact nature and situation of some morbid appearances. Generally a concave ocular lens will be required in order to gain a clear image of the details of the fundus. This will, however, depend upon the state of refraction of the patient's and surgeon's eye. The novice should have the patient's pupil dilated with a very weak solution of atropine, as this greatly facilitates the examination. But when the surgeon has acquired some dexterity, he must accustom himself to examine with the pupil undilated; for the atropine causes the patient much annoyance and inconvenience, and should, therefore, only be employed in exceptional cases.

*The ophthalmoscopic appearances of healthy eyes.*—The examination of the normal refracting media of the eye (cornea, lens, and vitreous humour) affords a negative result. It must, however, be remembered that the lens undergoes certain physiological changes with advancing years, which a careless observer might mistake for commencing cataract; for the lens substance becomes consolidated, and its nucleus assumes a yellow tint, which is especially marked by reflected light (oblique illumination), but on examining it in the erect image the lens will be found quite transparent.

The optic disc or papilla (entrance of the optic nerve) appears as a circular pinkish-white disc; being lighter and more glistening in dark eyes than in those of persons of fair complexion. It is sometimes oval, this being especially marked in astigmatism. Its outline is sharply defined, and on closer examination we can distinguish at its margin three boundaries or zones: 1, the inner, grey, nerve boundary; 2, the white sclerotic ring, which is most evident at the outer side; 3, external to this, the dark-grey line of the choroidal ring, which is somewhat irregular in shape, and often fringed here and there with deposits of pigment.

The internal half of the disc is redder than the external, which has a somewhat mottled, greyish-white appearance; the grey stippling being due to the nerve-tubules, which are seen in section, the white dots or lines being caused by the trabeculæ of the connective tissue. It is important to remember this physiological difference in the colour of the inner and outer portion of the disc, for inattention to this fact may lead to an

error in diagnosis; the normal redness of the inner half being perhaps mistaken for hyperæmia or inflammation of the nerve, or the whiteness of the outer portion for incipient atrophy.

The retinal vessels generally issue from or near the central portion of the disc, and their number, mode of division, and course vary considerably. The principal branches, however, always run upwards and downwards; as a rule, an artery and two veins pass upwards, and the same number downwards, but sometimes there are two arteries and two veins. Whilst numerous but small vessels run towards the inner side, only a very few minute twigs are sent to the outer. The retinal arteries may be distinguished from the veins by the fact that they are lighter, smaller, and straighter in their course, having often also a bright streak along their centre, which is due to the reflection of light from the cylindrical walls of the vessels. The blood-supply of the most anterior part of the optic nerve is maintained by three sets of vessels—1, by small twigs from the central vessels of the retina; 2, by branches emanating from a vascular circle situated close to the margin of the optic nerve, and formed by some of the short posterior ciliary arteries; 3, Leber has found that numerous arteries and some veins pass directly from the choroid to the optic nerve, so that the chorio-capillary system is directly continuous with the delicate network of vessels which surrounds the bundles of the nerve fibres. This anastomosis explains why there is so often a hyperæmic condition of the optic disc in inflammatory affections of the choroid.

Two physiological peculiarities are frequently observed in the optic disc—viz., spontaneous, or very easily producible, pulsation of the retinal veins, and physiological excavation of the optic nerve. 1. The venous pulsation is almost always confined to the disc, and is characterised by an alternate increase and diminution in the calibre of the vein; the emptying commencing at the centre of the disc and extending to the periphery, whereas the refilling is centripetal. Slight pressure on the eyeball with the tip of the finger renders the pulsation more marked. Whilst spontaneous venous pulsation is quite physiological, it must be remembered that it is otherwise with spontaneous pulsation of the retinal arteries; for this is always a pathological phenomenon, depending on an abnormal increase in the intra-ocular tension. 2. The physiological excavation may be at once recognised by its being limited more or less to the centre of the disc, and not extending to its margin. A white, glistening, oval or round spot is noticed at or near the centre of the disc, surrounded by a reddish zone, which varies in breadth with the size of the excavation; if the latter is small, the zone will be wide, and *vice versâ*. The edge of the white spot (excavation)

generally slopes gently, and is only steep or abrupt if the cup is funnel-shaped. If we trace the course of the retinal vessels from the periphery of the disc towards the centre, we find that they describe a more or less acute curve as they pass down over the margin of the excavation. This curve is, of course, the more abrupt and considerable the deeper the cup.

Leaving the optic disc, the beginner must next attentively study the general appearances of the background of the eye (fundus oculi); and observe the mode and distribution of the retinal vessels, and the difference between these and those of the choroid. The bright-red colour of the fundus is due to the reflection of the light from the bloodvessels of the retina and choroid, more especially the latter, and not at all to the retina itself; for as the latter is very translucent, it reflects but very little light, and is quite invisible in the eyes of very fair persons. In very dark eyes it appears as a thin grey film or veil spread over the fundus, being especially apparent around the disc and yellow spot.

The great variations in the appearances of the fundus which are observed in persons of different complexions should also be attentively studied. In dark individuals the fundus appears of a rich reddish-brown tint; and if the epithelial layer and the stroma of the choroid are rich in pigment, the choroidal vessels may be completely hidden, and only the retinal vessels be apparent, dividing and subdividing into numerous branches, which become smaller and smaller towards the periphery. If the epithelial layer contains but little pigment, but the stroma is richly pigmented, the appearances will be totally different; for then the choroidal vessels will be distinctly visible, appearing as bright-red bands or ribbons divided by dark islets or intervals, the intervascular spaces. If the stroma is light, the epithelial cells appear, with a high magnifying power, as small, circumscribed dots, uniformly strewn about the fundus, and giving the latter a granulated appearance. In the eyes of very fair persons (*e.g.*, albinos), the fundus is of a pale-red tint, and the choroidal vessels present a very striking appearance, being visible to their smallest divisions.

The examination of the yellow spot (macula lutea) generally affords a negative result; but in those cases in which it can be seen, it will be found that in dark eyes it appears as a largish dark-red spot with a small white dot in the centre, the foramen centrale. In light eyes it has a bright-red tint, the foramen centrale resembling a small light circle. The retinal vessels course round the yellow spot, sending a few twigs towards it, but leaving its centre free.

The beginner should always proceed on a certain system in his ophthalmoscopic examination, for by doing this he will be

guarded against overlooking anything. He should therefore examine every eye in the following manner:—First, with the oblique illumination; secondly, in the erect image; thirdly, in the reverse image.

The examination with the oblique illumination is of great use in ascertaining the condition of the more superficial parts of the eye—*i.e.*, the cornea, iris, pupil and crystalline lens, and even the anterior portion of the vitreous humour. It is to be thus conducted:—A lamp having been placed somewhat in front and to one side of the patient, at a distance of from two to two and a half feet, and on a level with his eye, the light is to be concentrated upon the cornea, iris, or crystalline lens, by a strong convex lens of two or three inches focus. The observer's eye is to be placed on one side of the patient so as to catch the rays reflected from the eye of the latter. To enlarge the image the surgeon may employ a second lens as a magnifying glass.

When the examination with the oblique illumination is finished, the surgeon must examine the refracting media in the erect image, and also ascertain the state of refraction. In this proceeding the mirror alone is used, the surgeon placing himself at a distance of ten or twelve inches from the patient. The latter should be told to look in various directions, in order that the more peripheral portions of the lens may come into view and any marginal opacities be detected. Moreover, these movements cause any vitreous opacities to be shaken up and to float through the field of vision. Next as to the refraction; if there is a high degree of myopia or hypermetropia, this condition may be readily detected by the fact that we can see the details of the fundus—*e.g.*, the retinal vessels, at some distance in the erect image. For instance, if, in a case of high myopia, we regard one of the retinal vessels on the disc or retina, and move our head slightly to one side, we notice that the image moves in the contrary direction; if we move to the right, it moves to the left, and *vice versâ;* so that we obtain a reverse image of the fundus. In hypermetropia, on the other hand, just the opposite occurs, for the image moves in the same direction as the observer. In emmetropia we do not with the direct method gain any image of the fundus at some distance, which is in part due to the great diminution of the visual field, but chiefly to the rapidly increasing enlargement of the object when the eye is examined at a distance, which is so great that in the area of the field no single vessel can be seen sharply defined, but only appears as a red reflection in the pupil. Mr. Couper has lately shown that with a weak concave mirror (of thirty inches focus) cases of mixed astigmatism may be easily diagnosed in the erect image; in such cases an inverted or erect image becomes alter-

nately visible according as the observer views the fundus through the meridian of the greatest or the least curvature. The state of refraction may also be diagnosed in the reverse image. In the *emmetropic* eye the reversed enlarged image of its fundus is formed in front of the object lens, and at its focal distance. In *hypermetropia* the emerging rays are divergent, and hence the image is formed further off than the focal distance, and is consequently larger than in emmetropia. In *myopia* the emerging rays are convergent; the image is therefore formed nearer than the focal distance, and is consequently smaller. Mr. Hutchinson has, moreover, pointed out that the existence of hypermetropia or myopia may also be recognised in the reverse image by observing whether the size of the disc undergoes any change when the object lens is removed further from the eye; for in hypermetropia it diminishes and in myopia it increases in size as the lens is removed to a greater distance.

I cannot too strongly impress upon beginners the importance of closely and minutely examining a great number of healthy eyes, so that they may become thoroughly conversant with the physiological appearances of the fundus, and the numerous peculiarities that may occur within normal limits. To those who have not the opportunity of examining many human eyes, I would recommend the use of Perrin's artificial eye. It consists of a brass artificial eye, fitted in front with a plano-convex lens, in the situation of the cornea. This lens is covered with a black metal cap, having a central aperture corresponding to the pupil. There are two of these caps; the one has a very small central aperture, which corresponds to the normal size of the pupil; the other a larger opening, like a widely-dilated pupil. There are, also, three lenses of different refracting power, thus enabling us to convert the eye into a hypermetropic, myopic, or astigmatic one. The posterior half of the eye opens, so as to admit of the insertion of a papier-maché cup or disc, coloured so as to represent the appearances of the healthy fundus, or of some pathological condition. There is a series of these coloured discs, illustrating many of the morbid ophthalmoscopic changes of the fundus.

[Before commencing any ophthalmoscopic examination of the fundus oculi, it is always necessary to examine the condition of the refracting media, both with the oblique illumination and in the erect image. In examining the lens or vitreous humour, it will be advisable for the beginner to have the patient's pupil widely dilated with atropine.]

With the oblique illumination opacities in the refracting media will appear in their true colours; opacities of the lens, for instance, look like grey or white lines or patches on a dark background; whereas in the erect image they will appear as

dark spots on a red background, for, as their surfaces reflect but little light, they will be seen in shadow. There is no difficulty in judging of the exact position if the opacity is situated in the cornea, the capsule, or the lens. But in the vitreous humour it is less easy to estimate the depth at which it is placed. This is best decided in the following manner:—The observer, employing the erect image, should look straight into the eye, so that his visual line passes through the turning-point of the patient's eye (which corresponds nearly to the posterior pole of the crystalline lens). Now if the eye under observation is moved about in different directions, the turning-point and the corneal reflection will alone remain stationary. Hence any opacity situated exactly at the turning-point will remain immoveable; whereas if it is situated in front of this point it will move in the same direction as the cornea; if behind it, in a direction opposite to that of the cornea. The further the opacity lies from the cornea, the greater will this movement be. If the object lies deep in the vitreous, just in front of the retina, the surgeon should examine the eye in the reverse image, and on slightly moving the object-lens from side to side he will notice that the further the object is from the retina, the more marked will be its movement in the same direction as that of the lens.

*The cornea* is best examined with the oblique illumination, especially for determining the exact situation, size, and nature of foreign bodies, ulcers, or opacities. A second lens, or even a miscroscope, may be used for the purpose of magnifying.

*Conical cornea* may easily be recognised without any aid, if it be considerable in degree; for, on looking at it from the front, the centre of the cornea will appear unusually glistening and bright, as if a little tear-drop were suspended from it. The shape and size of the conicity become still more apparent on regarding the cornea in profile; but in the erect image even slight degrees of the affection may be at once detected. For on illuminating the cone we receive a red reflection from the apex, which gradually becomes shaded off and darker towards the base; so that the central bright spot is surrounded by a dark zone, which is again encircled by a red ring. If we throw the light at different angles on the centre of the cone, that side of it which is opposite to the light will be darkened. On examining the fundus in the reverse image, a considerable parallax is noticed; for, on moving the object-lens to and fro, certain portions of the disc and of the vessels upon it will be displaced and distorted, whilst others remain unchanged.

*The anterior chamber and iris.*—Deposits of lymph, effusions of blood, or foreign bodies in the anterior chamber are best seen with the oblique illumination. The same is the case with morbid changes in the tissue of the iris, or deposits of lymph at the

edge or into the area of the pupil. Whereas the examination in the erect image will be found very useful in detecting any gaps in the iris, or any detachment of the latter from its ciliary insertion; for in such cases we shall obtain through the gap a bright-red reflex from the fundus, it being taken for granted of course that the media behind are transparent.

*The crystalline lens.*—A fully-formed, mature cataract is readily distinguished with the naked eye as a whitish, opalescent body in the area of the pupil; but it is different with incipient cataract, especially if it be but slight in degree and confined to the margin of the lens. Both the soft and hard variety of cataract generally commence at the periphery. In *incipient cortical* (soft) *cataract* we notice, in the erect image, dark, well-defined stripes intersecting the red fundus, and radiating from the periphery to the centre; between them there are generally smaller stunted stripes, or little opaque patches. The interjacent lens-substance is at first transparent, but gradually the cloudiness becomes more general and diffuse. With the oblique illumination the opacities assume a greyish-white tint, the stripes being broad, white, and often opalescent; but there is no yellow reflex, and this is of importance, as proving the absence of a more or less hard nucleus. In *fluid cataract* the opacity is homogeneous, of a milky-white or dirty-grey colour. and reaches quite up to the anterior capsule; and there are no opalescent stripes. The *nuclear or hard senile cataract* is met with after the age of thirty-five or forty, and, as a rule, also begins at the margin in the form of small narrow stripes and spots; the opacity gradually increases and assumes towards the centre a yellowish colour, which indicates the presence of a hardish nucleus. The larger the area yielding this tint, and the darker the colour, the larger and harder is the nucleus. If the opacity commences at the posterior pole of the lens, the general condition of the eye must be carefully examined—viz., the sight, the field of vision, the eye-tension, and the opthalmoscopic appearances of the fundus, if the latter be still visible, for this form of cataract (posterior polar cataract) not unfrequently occurs in the later stages of certain affections of the choroid and retina.

*Lamellar cataract.*—It is very important, in a practical point of view, to recognise this form, as it is often best treated by the formation of an artificial pupil, without any interference with the lens itself. On dilating the pupil with atropine, and examining the lens in the erect image, a circular, uniform, dark opacity (from two to two and a half lines in diameter) will be noticed in the central portion of the lens, surrounded by a more or less clear red zone. The cataract, in fact, consists of a layer of opaque lens substance lying between the transparent nucleus

and a clear portion of the cortical substance. With the oblique illumination the opacity appears of a uniform light-grey tint, sharply defined, and surrounded by a margin of lens-substance, which may be either quite transparent or is here and there studded with small opacities. If this marginal zone is sufficiently clear and broad to permit of good sight when the pupil is widely dilated with atropine, an artificial pupil should be made so as to enable the patient to see through this clear portion, and the lens itself is not to be operated on.

*Capsular opacities.*—The anterior capsule of the lens is often the seat of deposits of lymph due to iritis. These may be confined to the edge of the pupil, leaving the central area free; or the latter may be covered by a more or less thick film, or blocked up by a dense nodule of lymph. Again, after an operation for cataract, the pupil may be occluded by portions of wrinkled opaque capsule, enveloping perhaps remnants of cataractous lens-substance. *Capsular cataract* is distinguished by its white chalky appearance. Strictly speaking, the term is incorrect; for, though the capsule may be thickened and wrinkled, it remains transparent, the opacities being due to deposits on its inner surface. In irido-choroiditis, there is often great proliferation of the intra-capsular cells, subsequently perhaps undergoing fatty and chalky degeneration, which also extends to the lens. *Anterior central capsular cataract* consists of a small opacity occupying the centre of the pupil. When it is very prominent and elevated above the level of the capsule, it is termed pyramidal cataract. It may be congenital, but is more frequently formed in early childhood, in consequence of a perforating ulcer of the cornea.

In cases in which it is doubtful whether or not the crystalline lens is present, and if neither the oblique illumination nor the ophthalmoscope enable you to arrive at an exact conclusion, the *catoptric test* must be employed. This test depends upon the three images which may be observed in a normal eye when a lighted taper is moved before it. Two of these images are erect; the third is inverted. The erect images are produced by reflection from the surface of the cornea and the anterior surface of the lens; the inverted one by the concave posterior surface of the lens. The first two move in the same direction as the candle; the last in the opposite. Now, if the lens is absent from the pupillary area, the lenticular reflections are of course wanting. Moreover, the normal eye will in such a case be extremely hypermetropic. If the lens has been dislocated into the vitreous humour, its situation can be easily recognised in the erect image, for it will appear as a dark lenticular body lying in the lower or lateral portion of the vitreous. If there is only partial displacement of the lens, and not a complete dislocation,

its free edge will be noticed as a dark, sharply defined curved line, traversing the red fundus, and forming the outline of the transparent or opaque lenticular disc.

*Diseases of the vitreous humour.—Inflammation of the vitreous humour* (hyalitis) is, as a rule, a secondary affection, supervening upon inflammation of the retina, choroid, &c. But it appears more than probable that it may also occur idiopathically, and without any perceptible participation of the other tunics of the eye. The progress of hyalitis is best studied when a foreign body (*e.g.*, chip of steel) becomes lodged in the vitreous; for, with the opthalmoscope, we generally find that the vitreous in the vicinity of the foreign body soon becomes hazy, and the latter surrounded by a greyish veil, which increases in density and thickness the more connective tissue is developed, and assumes a yellow tint if suppuration sets in. In *simple hyalitis* the vitreous is diffusely clouded, the details of the fundus hidden by a grey film, and the sight greatly affected. There are also dark membranous or thread-like films, which are either fixed, or float about when the eye is moved. In the *suppurative* form a creamy yellow reflex is often noticed from the anterior portion of the vitreous, which becomes very apparent with the oblique illumination.

*Opacities of the vitreous* are best observed in the erect image, the patient being directed to move his eye quickly and repeatedly in various directions, which will cause the opacities to be shaken up and float about. The opacity may be diffuse and extend over the whole fundus, or it may be chiefly confined to the centre, whilst the details of the fundus are distinctly visible at the periphery. But together with the diffuse opacity we generally observe dark, membranous, filiform or reticulated opacities, which are either fixed or floating, and which may be due to blood effusions, to pathological changes in the vitreous cells, or to the formation of connective-tissue elements. To the patient they appear as dark cobwebby spots or flakes, being the more observable the nearer they are to the retina. They occur most frequently in inflammation of the choroid and ciliary body, and especially in sclerectasia posterior. Not unfrequently the opacities of the vitreous are the first symptoms of inflammation of the choroid; and it is not till a later stage, when the vitreous has again become transparent, that the (perhaps slight) inflammatory changes of the choroid are detected. I have often observed this in cases in which the disease was dependent upon syphilis. Effusions of blood into the vitreous frequently cause these opacities, which then yield a peculiar bright-red reflex. But if the hemorrhage is very extensive and diffuse, it may be impossible to illuminate the fundus, and it yields no reflex, but looks quite dark. The chief danger of hemorrhage into the

vitreous lies in the fact that it is apt to recur again and again, and may thus lead to detachment of the retina, glaucomatous complications, or atrophy of the eyeball. In the treatment of vitreous opacities we must be chiefly guided by the cause, and by the fact whether they are due to inflammatory affections of the deeper tunics of the eye or to blood-effusions. In cases in which the opacities depended upon insidious choroiditis or iridochoroiditis (especially syphilitic), I have often derived very marked benefit from mercurial inunction, combined with the application of the artificial leech to the temple. The latter remedy especially accelerates the absorption of the opacities by preventing and relieving congestion of the choroidal and retinal circulation. If the patient is weak, dry-cupping may be substituted.—*Lancet, March* 11 *and April* 15, 1871, *pp.* 331, 497.

## 62.—ON THE NATURE AND TREATMENT OF SIMPLE GLAUCOMA.

By the late Professor VON GRAEFE, Berlin.

*Simple Glaucoma, its nature.*—The doubt felt in former days whether the same process could excite both the inflammatory changes of typical glaucoma and simple increase of tension, has now been partially removed by our fuller knowledge of secondary glaucoma and by recent alterations in the theory of inflammation. When we see in secondary glaucoma, for example after displacement of the lens, one day simple increase of tension, and the next day inflammatory cloudiness of the media, alternating just as the cause acts more or less intensely, we may be certain of the essential identity of both forms and that they are due to modifications in the intensity and duration of the cause. If we admit the form with cloudiness of the media to be inflammatory, we cannot, I think, deny that the other, which is so closely allied, is also of the same nature, although, for the sake of distinction, it may be very well be called the "non-inflammatory," or "simply secretory" form. The supposition of a simple difference in degree is supported by Cohnheim's theory, according to which inflammation is essentially a species of abnormal secretion. If the cause acts rapidly and powerfully on the vessels, so that the cellular elements pass abundantly into the ocular media, the haziness becomes perceptible; whilst if the cause acts more slowly and less intensely, the cloudiness is not sufficient to be recognised. A very little addition will then render it visible, such as, for example, is produced by the congestion after meals.

The real difficulty in respect to glaucoma simplex arises from our *ignorance of any immediate or remote cause.* In secondary glaucoma we find, in one or the other tissue, a stimulus exci-

ting hypersecretion, and as the former is usually permanent, so also is the effect. If temporary arising for example, from a swollen lens, the tension may also again diminish, though if the latter has attained a certain degree and duration, it will persist after removal of the cause; it is doubtful whether this is due to persistent irritation of the secretory nerves or to a continued pernicious reaction by the anatomical results of the increased tension. We can understand how a typical inflammatory glaucoma may leave changes, such as contraction of the emergent veins or alterations of the ciliary nerves, so that, even without a recurrence of the inflammation, the pressure persists and leads to destructfon. When there are recurrent attacks of inflammation, we feel more acutely our ignorance of their causes, though, as in other forms of recurrent inflammation, we may refer them to remote conditions and processes in the organism. But when, as in glaucoma simplex, the pressure augments very gradually for months or years (though depending anatomically on the same, but less intense, processes), always advancing and never diminishing, we naturally ask where is the continued stimulus that causes this course. Even if we admit an idiopathic affection of the nerves from some extra-ocular source, it would still be very surprising that the tension never diminishes again. *A priori* it is far more probable that the cause is *intra-ocular*.

*Variations in its Course.*—The results of *treatment* force us to admit a *latent intra-ocular* cause for glaucoma simplex and lead to the opinion that this cause is subject to fluctuations. Whilst the operation has a perfectly uniform effect in simple inflammatory glaucoma, whilst in secondary glaucoma its various results may be naturally deduced from the nature of the primary cause, we are obliged to admit that cases of glaucoma simplex vary extremely in their ultimate course, even when the pre-existing conditions appear to be exactly similar. The result is often very satisfactory in a relative sense, when the operation is performed at a late period; I mean that after six or seven years the patients continue to see decidedly better than at the time of the operation; in some, operated at an early period, the vision remains for many years in the same state, then gradually deteriorates; in some, again, the disease is not rendered perfectly stationary; and finally, some, fortunately very rare, patients become rapidly blind after the operation. When we accurately determine and record the tension, we find that these differences more immediately depend upon its variations. Thus iridectomy reduces the tension directly and permanently to its normal amount in most cases of simple glaucoma. It reduces it considerably, but does not render it quite normal in others; the vision usually continues in the same state, the eye acquiring, as it were, a

sort of adaptation to a moderate increase of tension; yet not unfrequently the sight becomes again worse after a time, or the pressure again increases slightly and gradually, until in either event by a second operation, which I shall afterwards describe, it is rendered permanently normal and the vision secured. In a third series, the tension is normal or but little increased soon after the operation, whilst at a later period there are from time to time considerable increases, which do not always permanently yield to a second operation. Lastly, iridectomy is sometimes followed by greater instead of less tension, the result being that vision is lost by progressive impairment or by a sudden attack resembling acute glaucoma.

Hence it would almost seem as if iridectomy had such an uncertain effect in simple glaucoma as to render its indication doubtful. This conclusion is found to be quite erroneous when we consider the numerical distribution of the varieties described. It has been already mentioned that in more than half the cases, a single operation renders the pressure permanently normal, and not only preserves, but even gradually improves, the vision for a long period. The second category includes fully one-fourth of the cases, and though our object cannot be said to be fully attained, yet the result is generally satisfactory: it may be necessary to repeat the operation in some few cases in order to secure permanent success. A second operation sometimes stops the progress of the blindness in the third category where the disease recurs; it at all events renders the deterioration slower in comparison with the spontaneous course. The last variety alone is thoroughly unfavourable, blindness being hastened by the operation. Yet this "malignant" course is extremely rare: I have seen it fully developed only five times in some hundred cases of simple glaucoma on which I have performed iridectomy. Judging not only by my own practice but also by that of others, it does not form two per cent. of the cases of glaucoma simplex submitted to operation. Now an operation that gives permanent protection from blindness in more than 90 per cent., renders the course slower in the majority of the remainder, and does harm in less than two per cent. is indisputably indicated, though exceptional bad results, such as never occur in an analogous manner in inflammatory glaucoma, may necessitate prudence in the prognosis.

The effect on the tension is reflected in a very instructive manner in the behaviour of the eye, even during the first few days, in part *immediately after the operation*. Whilst, as already mentioned, the tension in inflammatory glaucoma is rendered uniformly normal, with the exception that it may be moderately increased during the beginning of the healing process and that there sometimes remains a slight excess, quite compatible

however with permanent success, the wound rapidly heals, and the anterior chamber is refilled immediately after the operation, or in a few hours, exceptionally not till some days afterwards, the state is quite different in simple glaucoma. If we make it our practice to examine each eye immediately after iridectomy by palpation, of course with the necessary delicacy, we shall find that the resistance varies much in glaucoma simplex. Some eyes are almost as soft as (even softer than) the average normal eye after it has been punctured, in others the tension remains greater than it ought to be in the normal eye before it is punctured. We find every degree of abnormal tension between the two. Now the anterior chamber will not be rapidly reformed, when the tension immediately after the operation is considerable; the chamber will not refill till the third, fourth day, or later, even when the tension does not reach quite such a height, though decidedly exceeding that of a punctured eye. If, on the other hand, the resistance is but little greater than that of a punctured eye. The chamber is permanently refilled (the incision being made with the lance-knife) immediately or very soon after the operation. The unfavourable results, noted above, occur when the chamber is slow in refilling. The pressure may even increase somewhat for a few days, during which time the iris and lens are pressed against the cornea, which consequently becomes somewhat hazy in most cases, the eye waters, the congestion is pericorneal and peculiar, due principally to enlargement of the anterior conjunctival veins which do not run in straight lines in their usual manner, but bend at obtuse angles; their inosculations with the episcleral vessels, which are also much congested, are likewise very noticeable. Palpation especially of the ciliary region near the wound, is painful, and the patient complains of more or less ciliary neurosis and loss of sleep; vision decreases in an alarming manner. The chief danger, when the anterior chamber does not refill, is caused by a *consecutive increase of tension (with symptoms of irritation)*, on which the other symptoms obviously depend. The danger is less when there is *no irritation*, that is, when pain on palpation (not spontaneous ciliary neurosis, which is caused directly by the increase of tension), lacrymation, and marked congestion (of which the character is determined ultimately by the state of the tension) are absent; the eye usually becomes softer in some days, and though the chamber does not refill till the second week or later, yet afterwards the course is tolerably satisfactory in most cases. If there is no irritation and the tension is but little increased whilst the chamber keeps empty, we may expect that the aqueous will begin to appear again in a few days, that a little later the chamber will be quite normal, and that the result will be satisfactory.

Though a signally *malignant course* is observed in less than 2 per cent. of the cases of single glaucoma treated by operation, yet in a much larger proportion, about 10—12 per cent., the chamber is slow in refilling (after the second day), owing to the tension being relatively too great. About one-half of these present symptoms of irritation, which are, however, generally slight, and fortunately disappear under proper treatment, as the chamber refills and the tension decreases. Yet here we find almost all the cases in which the reduction of tension is incomplete or is followed by a fresh increase.

Returning to the "malignant course," we may add that it presents various forms. The irritation may steadily increase during the first few days after the operation, the tension may become so great that the eye, though the anterior chamber is empty, presents a stony hardness, and even the last trace of vision is lost. The cloudiness of the posterior surface of the cornea and a diffuse haziness of the vitreous, which certainly exists, prevent any accurate examination of the fundus. No vision ever returns, though some weeks later the pressure decreases and a shallow chamber reappears. We then find a tolerably diffuse, delicate opacity of the vitreous or retinal separation: I have seen the latter follow in two cases, whilst the tension was decreasing; once indeed the vitreous had been punctured. Sometimes the anterior chamber is permanently obliterated owing to adhesion of the iris and cornea, though the tension ultimately decreases. In other cases the symptoms of irritation may not be so great, or they may remit to a certain extent, yet the chamber continues empty, the tension persists, there is more or less abnormal irritability, and the vision is gradually lost, just as in progressive glaucoma, the field contracting, &c. There can be no doubt that the operation excites a fresh and more acute glaucomatous process, which does not present its ordinary symptoms simply owing to the complete absence of the aqueous humour. Judging by the painfulness of the ciliary region near the wound, this process would appear to spring from irritation of the ciliary body; a further ominous feature is the sympathy of the other eye, even after it has been already operated with success. I have failed to find any reason for this course, either in the manner of performing the operation, or in the personal peculiarities of the patient.

I wish to call attention to the fact that I have seen this course exclusively in *glaucoma simplex* strictly so called (apart from a glaucoma secondary to displacement of the lens, also without haziness of the media), *i.e.*, only where no periodical obscurations of the media could be found by careful and repeated examination. I may say the same of the analogous cases in which the chamber is late in refilling, where the tension remains in-

creased for a long time after the operation, and where there are symptoms of slight irritation. I have never seen such a course in cases of inflammatory or subacute glaucoma with periodical cloudiness of the aqueous humour. I must also add, that all the eyes in question were *very tense* before the operation. There appeared to be in some a peculiar cloudiness of the subconjunctival connective tissue, so that the swollen muscular or anterior ciliary vessels presented a peculiarly dull hue; this point, however, requires further attention.

Hence there must be a difference—and here we return to the subject with which we commenced our remarks—between apparently similar cases of simple glaucoma, owing to which the very same operation may diminish the tension immediately and sufficiently, imperfectly or not quite permanently, or on the other hand may excite a sudden and pernicious increase. We have already learnt that there are cases of secondary glaucoma clearly arising from an intra-ocular cause, in which the latter can be determined only by the antecedents. Such is the form that occurs with posterior cortical cataract after serous choroiditis, at a time when its principal effect, diffuse obscuration of the vitreous, has passed away; such also is the one following retinal hemorrhage at a time when every trace of the latter has disappeared. Just as in these cases the cause of glaucoma is latent when the operation is performed, so there may be causes of simple glaucoma, which escape our notice; a great part indeed of the structural and circulatory changes, which affect the eye, are not directly accessible to our present means of examination. We have already spoken many times about choroiditis latens, *i.e.*, about choroidal changes which cannot be shown objectively at every stage, but which are demonstrated by the course of the disease. It is in this way alone that we can account for the peculiar reactions after operations on certain forms of cataract. It is quite certain that there are diseases of the vitreous without any ophthalmoscopic symptoms whatever; thus when the lens is very mobile, passing sometimes into the vitreous, sometimes into the anterior chamber, we cannot doubt that the vitreous is unnaturally fluid, and yet we are assisted to this conclusion only by accident.

Important parts, such as a portion of the ciliary organs and the sclera, which are very likely to participate in the genesis of glaucoma, cannot be directly examined. The absence of inflammation in simple glaucoma harmonises with the view that it arises from a tissue with very sluggish nutritive changes and a peculiar vascular torpidity. The differences in the course of the disease and in the results of treatment, may readily be imagined to be due to the implication of particular parts of the sclera, a membrane largely connected with adjacent tissues, and through

which various vessels and nerves pass. As in other disturbances of innervation we attend especially to those portions of the nerve passing through bone or tendon (numerous cases of neuralgia and paralysis are now-a-days referred to periosteal irritation at those spots, &c.), so in respect to the ciliary nerves their passage through the sclera seems to me to deserve a thorough examination. I should suspect senile rigidity or pathological contraction of the sclera, because of the interference with nerves containing secretory fibres, and not because it directly compresses the contents of the eyeball, nor yet because the venous circulation is impeded or obstructed (see Stellwag, Der Intraoculare, &c., der Iris, p. 43).

It is a doubtful proceeding, when we are without positive data, to employ hypotheses. Yet, apart from the fact that they are likely to give the necessary impulse to investigation, it seems desirable to us to form such views of simple glaucoma as will not contradict current pathological doctrines and analyses in respect to other forms of the same disease. For these reasons we must suppose, in my opinion, that there is a permanent stimulus in the eye; we are also obliged, owing to the entire similarity of the course and effects of treatment, to consider the disease a *secondary glaucoma* with a varying or at least not uniformly localised *intra-ocular cause*. The cause itself is still doubtful, yet for various reasons special attention should be directed to the sclera in future clinical and anatomical researches.

Glaucoma simplex affects, almost without exception, both eyes successively, indicating that the cause also is symmetrical. Iridectomy is the principal means of treatment. Paracentesis has no permanent effect, though it possibly may be of use in regard to the prognosis. If it reduces the tension nearly to that usually found in a punctured eye, we may be certain that iridectomy will have a permanent effect, and that there is no tendency to a malignant course, or even to an insufficient result of the operation. If again the hardness continues relatively great, the anterior chamber will be slow in refilling and the course will be threatening, though malignant only in a minority of patients. Possibly a methodical repetition of the puncture would reduce the tension, and thus, as the puncture itself has no permanent effect, prepare the way for iridectomy. I cannot, however, yet say whether such a proceeding will succeed, or whether in cases where iridectomy is followed by a malignant course, paracentesis itself would be attended with the same risk. As to the iridectomy, I need scarcely repeat that, in order to reduce the tension with the greatest possible certainty, the usual rules about size and peripheral position (and the avoidance of adhesions, to which we shall return at the end of this essay) must be strictly observed in cases of simple glau-

coma. The disease differs in this respect from inflammatory glaucoma, where carelessness in the operation is often enough followed by no injurious effect. The after-treatment, which presented no peculiarity in the inflammatory form, here requires much consideration. The tension of the eye should be carefully examined immediately after the operation, or if chloroform is used, as soon as its effects have disappeared. When the tension scarcely exceeds the average resistance of a punctured eye, we may expect that the anterior chamber will be soon refilled, and even should this not occur for some days, that the course will be satisfactory with the usual treatment. The continued use of the charpie-bandage is advisable for the prevention of cystoid cicatrisation; atropine may also be applied from the second day with advantage. On the other hand, if the tension exceeds considerably that degree, the eye must be simply closed with plaster. I am firmly convinced that a *bandage applied to a relatively tense eye, decidedly increases the after-danger*. The slightest pain should be opposed by a subcutaneous injection of morphia in the temple. If next morning the pressure is not decreasing —the chamber will, of course, still continue empty—occasional tepid fomentations with chamomile infusion, a purging powder of calomel, and in plethoric people venesection, are advisable. The fomentations, powder, and subcutaneous injection should be repeated during the following days according to circumstances. If the pressure is once decidedly diminished, we may calculate on the chamber being soon reformed, and on the course being tolerably satisfactory, unless the vision has already exceptionally decreased. I do not recommend atropine in these cases during the first few days, because, with the existing tendency (as in chronic glaucoma) it may excite an acute increase of tension. These means during the first few days not only counteract the tendency to a malignant course, but conduce to the completeness and permanency of the reduction of tension, on which the future fate of the eye depends. As already mentioned, the very eyes in which the pressure is insufficiently reduced soon after the operation, are usually the most prone to ulterior deterioration or recurrence.

*Supplementary Iridectomy*.—If, nothwithstanding all this care, the vision becomes again worse after it has been stationary for many months, and the tension increases or recurs, the first means, provided the latter is not very great, should be occasional local blood-letting by the artificial leech. I have already on a former occasion recommended this treatment to be pursued some months after the operation, and can now repeat, what I then said, in respect to many cases of simple glaucoma (and of secondary glaucoma from sclerotico-choroiditis posterior). The favourable effect of this treatment is again a reason for believing

that this affection is due to some intra-ocular irritation. Should this means fail, it might be a question whether we should have recourse to paracentesis, methodically repeated; my own trials, perhaps not sufficiently carried out, have not given any decided results, and for some years I have abandoned them, because I found that excision of a diametrically opposite piece of iris very decidedly reduced the tension of the eye. I shall treat this subject, already repeatedly mentioned in this paper, specially here, because it is most frequently indicated in simple glaucoma. Many comparative trials in the years 1865-67 proved that excision of the opposite piece of iris had a far more decided effect on the tension than that of the adjacent (even extremely large) portion of iris. These trials were made a few times when both eyes were in almost exactly the same state; the result was decisive, for excision of the adjacent piece had afterwards to be supplemented by that of the opposite portion of iris. I must indeed confess that I have become dubious whether excision of the adjacent piece has any influence over the tension; it is at all events very slight, and will generally not suffice in cases where the first iridectomy has had no sufficient or permanent effect. On the contrary, I could adduce at least a dozen cases, where excision of the opposite piece has had a permanent effect after failure of the first iridectomy.

*The Operation for Glaucoma.*—I shall conclude with some remarks on this subject. I mentioned at the commencement of this essay, that the rule to excise a large piece of iris up to the periphery is not of equal value in all cases. I consider it of great importance for our object, and it has a certain, though limited, relation to the older rule, that the iris should be carefully and completely removed from the wound, *for any inclusion of it in the cicatrix will certainly increase the secretory irritability of the eye.* Had I to choose between a coloboma that does not reach quite to the periphery, but in which the iris is free, and one perfectly peripheral, but in which one edge of the coloboma is largely included in the cicatrix, I should always prefer the former. The more peripheral the internal wound, the greater is the risk of including the iris. That an inaccurately performed operation will generally answer in very acute glaucoma, may be partly owing to the operator fearing in such a case to make a strictly peripheral wound, and thus more easily avoiding adhesion of the iris. It is necessary to make the internal wound sufficiently long in order that, with a large spontaneous prolapse, the iris may be excised without any excessive traction up to the periphery. I am more and more inclined to advise the operator himself to excise the iris, especially when he is not perfectly accustomed to his assistant. The latter, however skilful, can scarcely apply the convexity of the scissors so exactly to the

edges of the wound, or even within the wound itself, as the operator, who, using both hands, feels the tension of the iris. Not only must we take care not to leave a peripheral zone of iris (more than the unavoidable minimum) in the very act of excision—the latter need not, owing to the shortness of the wound, be *à plusieurs temps*, as in the operation for cataract—but we must also remember that one or other edge of the coloboma may be included in the angle of the wound. That this is the case, is shown by the corresponding side of the coloboma being not sufficiently long, and the angle of the sphincter being somewhat approximated to the wound. We must then act just as in the operation for cataract. Re-position may be effected by stroking the eye with the convexity of the caoutchouc spoon, and the same proceeding will serve to cleanse the wound of any small flakes of pigment. Should there still remain any shortening of one edge of the coloboma, a fold of iris is included in the angle of the wound, and must be excised. If blood in the anterior chamber impedes our view, the lids should be closed and gently compressed with a sponge dipped in iced water; the hemorrhage will stop in a few minutes, and the blood will escape, at all events sufficiently, if the wound is made to gape and the globe is gently compressed. *In no case should the operation be concluded till we are certain that the iris is completely released from the wound.* The prevention of *posterior* synechiæ, though of secondary importance, should also be attempted. The tendency to their formation may be generally recognised immediately after the operation, by a projection of the angles of the sphincter. When this is the state, I usually let atropine be applied a few times on the day after the operation; in other cases I do not commence with its regular use till the third day (excluding the cases in which the eyes continue excessively tense and threaten a malignant course).

Besides the stricter recognition of these principles, and the more exact indications in the various forms of secondary glaucoma, I consider the diametrically opposite iridectomy an important advance in the treatment. No progress of importance has been made in respect to the *theory* of the operation, notwithstanding many attempts. Old hypotheses are usually dragged forth, little as they avail for the explanation of existing facts. The unhappiest of all, the one referring the effect simply to the wound of the ocular capsule, is periodically brought forward, decked in some new formula, and recommended for adoption. Fortunately for our patients, practice advances independently on the foundation of empirical principles. No doubt, some day a satisfactory theory will be found, but in the meantime we must not be led away by transient hypotheses.—*Ophthalmic Hospital Reports, Feb.* 1871, *p.* 98.

## 63.—ON CATARACT.

By HAYNES WALTON, Esq., Surgeon to St. Mary's Hospital.

[A perfectly soft cataract is effectually and safely removed by opening the capsule and allowing the aqueous humour to come into contact with the opaque lens tissue.]

The freer the contact of the aqueous humour, the quicker is the cataract absorbed, but there is a limit to the degree to which such contact is admissible, arising out of the necessity for preventing some disturbing or destructive influences which would otherwise arise. Too much must not be done at once. It should be a great principle, in solution, to procure absorption of the cataract in its natural position. The less the lenticular matter is displaced, the less subject is the eye to irritation. The less, too, the capsule is torn in the early stage of the treatment, the less likely is it to contract adhesions to the iris, and, therefore, the more easy to be disposed of afterwards, if it block up the pupil.

There are two methods of operating—one through the cornea, and the other through the sclerotica. The corneal is more definite and simple, and inflicts less injury on the eye, and the needle is never out of sight. It is the one which I adopt.

At a first operation, all that is desirable is to tear through the centre of the capsule, and to penetrate and break the cataract to a small extent, but sufficiently to admit the aqueous humour to its texture. Dislocation of the cataract is to be avoided, but such displacement is very sure to occur at once, or subsequently if the capsule be much divided. This movement of the cataract, by which it falls into the posterior chamber of the eye, or the anterior, is very apt to cause inflammation, by which the process of absorption is delayed, or the eye even damaged. The same may be said of portions of the cataract escaping from the capsule. Swelling of the cataract always ensues, and is apt at all times to cause the wound in the capsule to enlarge; and, of course, the greater the length of the original tear or incision, the more likely is the wound to enlarge, because, according to the extent of the exposure of the cataract to the aqueous humour, is there increase of the swelling. The greater the atrophy of the lens fibres, the less do they swell. A healthy lens enlarges very much under such conditions. We have an opportunity, sometimes, of seeing the process in accidents to the eye which involve the lens. Another disadvantage of cataract swelling is pressure. The iris may be so affected, and much pain induced, and even disorganising inflammation developed.

During the whole of the period of absorption, the pupil should be kept dilated. By this, adhesions between the capsule and the iris may be prevented, and sometimes pressure

on the iris avoided. It has been supposed that expansion of the pupil may be the cause of dislocating all or some of the contents of the capsule; but this is erroneous. On the contrary, dilatation may be advantageous in causing any detached portion of the cataract to fall into the anterior chamber instead of into the posterior, the far better position, because inflammation is so much less likely to ensue when it is occupied.

Very slight irritation follows a well executed operation on an eye which is not complicated with some other disease, and there is not the necessity for that strict observance of quiet so essential in the operation for extraction. However, the patient should remain in the house for a few days, and bright light should be excluded. If acute inflammatory action arise, it must be regarded as traumatic, and treated as such. The eyeball invariably gets harder if the slightest inflammation supervenes; but this symptom need not create the least alarm, nor should it call forth any particularly active treatment under the fear of glaucoma supervening. Glaucoma never has a traumatic origin, It is difficult to understand why it is that the eyeball does not become hard in idiopathic inflammations, even when they are intense. An eye may become destroyed from rheumatic or syphilitic inflammation without ever getting hard.

The pain which always follows the operation for solution, but is generally very slight, must not be mistaken for the commencement of an attack of inflammation. So long as the objective symptoms of acute inflammation are absent, narcotics, and the application of cold lotions, need be employed. Vomiting sometimes ensues, especially if any portion of the cataract become dislocated. Ice broken in little lumps, and taken by the mouth, generally stops it.

*Results.*—I will now consider the effects of portions of the cataract in the chambers of the eye, because fragments may escape from the capsule in spite of all care. When the cataract has become very soft in its superficies, some of the degenerated matter generally falls into the anterior chamber, and is soon absorbed. If there should escape much of the broken cataract, the plan is to wait and see what happens. It might be absorbed without any disturbance. Should severe pain come on, the displaced material ought to be evacuated by an incision through the cornea, assisted by the spoon of the curette. If the entire cataract should fall into the anterior chamber, or the nucleus of it should follow the soft superficies, extraction is the rule; yet I should be inclined to wait for symptoms before I acted, because absorption might proceed uninterruptedly, but slowly. Agonising pain of a neuralgic character in the eye, forehead, and head, is the precursor of dangerous inflammation. When the cataract, or its nucleus, or a large fragment, has fallen into

the posterior chamber, much disturbance is likely to arise from pressure on the iris. The treatment is either to depress what is lying there, or to bring it into the anterior chamber with a cataract needle, and then to extract it.

The time required for absorption depends on the stage of the cataract, and the patient's age: the more the lens fibres have undergone atrophy, and the younger the individual, the quicker will be the removal. A single operation suffices in some instances, especially in infants, and it certainly will be sufficient when the lens has degenerated to fluidity; but for the most part repetition is necessary. If after the lapse of four or five weeks there is not evidence, in the flattening of the capsule and the concavity of the iris, that absorption is proceeding sufficiently fast, or if the aperture in the capsule should close, the operation is to be repeated. In such repetitions, the cataract tissue should be more freely broken up. At the last operation the anterior capsule should be torn across, with the object of clearing the pupil. If a sufficient interval between the operations be allowed, there are few cases which will not yield to two or three operations. Congenital cataract should be operated on before the eyeball begins to oscillate, and a child may be safely submitted to the operation after the first month of life.

The operation for solution is certainly the safest of all for the removal of cataract, as regards any immediate or secondary danger to the eye, and from none other is so much success to be got. It is one of the most scientific and beautiful operations in surgery. Not any one damages so little—that is, has so few drawbacks. It embraces all the superiority that we claim for subcutaneous operations, in general surgery, over surface wounds. It can be sufficiently well done by anyone with the least aptitude for operating. It is the easiest of all the operations for cataract. It is the appropriate one for soft cataract, beyond the possibility of a doubt. If any method of curing cataract without a surgical operation be ever discovered, absorption will be the process.

This old English operation, which removes the cataract and spares the eye the risks of extraction, is at this period apt to be neglected, because not sufficiently taught, in consequence of the love of novelty, the force of fashion, and enthusiastic admiration of some inferior foreign methods. I am quite sure that if it were one of modern discovery it would be universally hailed as the greatest boon which ophthalmic surgery had ever received. On some other occasion I will tell you about the operations which some surgeons employ for extracting soft cataract.

After the cataract has been removed the capsule may not roll up or contract sufficiently, but occupy the pupil and impair

vision. In such a case an operation must be undertaken, by which a sufficiently clear space can be made for the transmission of light to the retina. This is, in fact, making an artificial pupil, the rules for which will be given when I come to speak of making false pupils. All that I need say now is, before any such operation be undertaken, you should be sure that the capsule does obstruct vision. Trying the sight with test-type is the only way to discover the truth. A small bar of capsule passing across the pupil, or a piece of capsule jutting out a little beyond the margin of the pupil, does not necessarily interfere with vision.—*Med. Times and Gaz., Dec.* 31, 1870, *p.* 749.

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## 64.—HYSTERICAL CLOSURE OF ONE EYELID CURED BY GALVANISM.

By FRANK H. HODGES, Esq., House Surgeon to the Birmingham and Midland Eye Hospital.

A female servant, aged sixteen, was brought here from a country town on the 17th February, by her mistress, suffering from inability to raise the right upper eyelid. On inquiry, it was found to have existed upwards of six weeks, and to have resisted a variety of both external and internal remedies; the former consisting of large blisters across the brows, which were still unhealed, and, according to the patient's account, of galvanism to the temples. The mistress had been alarmed by the surgeon in attendance giving it as his opinion that the affection was due to disease of the brain. The appearance and age of the patient, together with the absence of any other implication of the third nerve, suggested hysteria as the cause; and this diagnosis was borne out by the fact that on raising the affected lid with the finger whilst the patient's attention was attracted by an examination of the sound eye, and then removing the support, the eyelids remained open until the patient, suddenly recollecting herself, again closed them.

My friend Mr. Maberley, a student of the Queen's Hospital, who saw the case with me, and who had about a month previously witnessed the immediate cure of hysterical aphonia by the sudden galvanic shock applied directly to the vocal cords by Dr. Morrell Mackenzie's apparatus, suggested that similar treatment would be applicable in this case, which was carried out as follows. The patient having been previously told that an immediate cure would result from the operation, one conducting wire of the galvanic battery was applied to the outer canthus, and, the instrument having been wound up to its full strength, the other wire was brought down upon the inner canthus. The effect was startling, the patient screaming and

clutching at her eye; and when the hand was removed the eyelids were found to have regained completely their natural action. To ensure a lasting impression upon the patient's mind, the operation was repeated, and she went away, having returned many thanks for regaining the power she had lost for so many weeks. Nothing has since been heard of the girl, and, as her mistress promised to bring her at once to the hospital should the symptoms recur, it is concluded that she continues well.—*Lancet, March* 18, 1871, *p.* 378.

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## 65.—CARBOLIC ACID IN OTORRHŒA.

By JOHN P. PENNEFATHER, Esq., London.

The following cases, as examples of the almost uniform good result attending the use of carbolic acid as an injection in chronic otorrhœa, may not be uninteresting to the profession. I have taken them at hazard from some hundreds which I have treated in a similar manner at the Royal Dispensary for Diseases of the Ear. In the few instances in which its application failed to entirely cure, it not only lessened the secretion considerably, but deprived it of the disgusting fetor by which it is usually characterised—a boon of no small moment both to the sufferer and the surrounding friends. The proportions in which I have prescribed it, are: carbolic acid, one drachm; glycerine, one ounce; distilled water, five ounces. I have never found it create the slightest irritation, and the only complaint I have heard was when a too vigorous application of the syringe forced the fluid through a perforated membrane into the mouth.

*Case* 1.—Wm. W. fell from a ladder four years ago. Had suffered from profuse, highly offensive discharge from his right ear ever since. On Nov. 13th was ordered to syringe with carbolic-acid lotion three times daily. Discharged, completely cured, on Nov. 26th.

*Case* 2.—Mary Ann W. has suffered from highly offensive discharge from right ear for three years, after scarlatina. On Nov. 12th was ordered to syringe with carbolic-acid lotion. Discharged, well, on Nov. 26th.

*Case* 3.—John W. W. Discharge from right ear for twelve years; attributes it to a severe cold. On June 17th was ordered to syringe with carbolic-acid lotion three times daily. Discharged, well, on July 1st.

*Case* 4.—Henry B., aged three years. Discharge from both ears for twelve months, after whooping-cough. On Oct. 28th was ordered to syringe thrice daily with carbolic-acid lotion. Discharged, well, on Nov. 18th.

*Case* 5.—Jane W. Discharge from left ear for ten years, after scarlatina, highly offensive. On July 6th was ordered to

syringe with carbolic-acid lotion three times daily. Discharged, well, on Nov. 25th.

*Case* 6.—Mary S. Discharge from left ear for fifteen years, after scarlatina. Complains much of its offensive character. On June 8th was ordered to syringe with carbolic-acid lotion four times daily. Discharged, well, on Nov. 18th.—*Lancet, Dec.* 3, 1870, *p.* 804.

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## 66.—OBSTRUCTIONS IN THE EUSTACHIAN TUBE.

By Dr. PETER ALLEN.

[The following is an epitome of a paper read before the Medical Society of London.]

The author described the methods most recently devised for overcoming obstructions in the Eustachian tube, especially such as result from catarrh, and also for inflating the tympanum, with the latest improvements made upon the instruments in use for these purposes. The subject was introduced by a brief sketch of the functions of the parts intended to be influenced, the different methods hitherto adopted, and their relative value. The method particularly dwelt upon, for its high practical value, simplicity, and general efficiency, was that named after its originator, the "Politzer method," which may be shortly described as follows:—The surgeon places the tube within either of the patient's nostrils, or, with Dr. Allen's most recently improved contrivance, simply presses the nasal pad gently and firmly against them. The patient then swallows, by degrees, a small quantity of water previously taken into the mouth; and the surgeon, at each successive act of deglutition, compresses the air-bag held in his own right hand. Currents of air are thus repeatedly sent into the naso-pharyngeal cavities, driving warm air into the tympanum through the Eustachian tubes, and overcoming at once any moderate degree of obstruction in the latter. The modifications of this appliance, introduced by Dr. Allen, consist of an interior valve, enabling the acts of inflation to be repeated as often as desired, without the necessity of removing the instrument from its position; and also of the double pad placed against the nostrils of the patient, thus dispensing with the rather disagreeable and troublesome procedure of inserting the tube into the nostril. Even young children do not cry, nor are other persons frightened, at this simple and easy mode of managing inflation. It causes no pain or distress, and the results are often at once so happy as to astonish the patient.

Personal testimony was borne by one of the Fellows present to the immediate relief afforded him, when suffering from catarrhal deafness, by Dr. Allen's use of the described apparatus.—*Lancet, Feb.* 18, 1871, *p.* 232.

## 67.—A POINT IN THE EARLY (CONGESTIVE) STAGES OF SKIN DISEASES.

By Dr. TILBURY FOX, Physician to the Skin Department of University College Hospital.

[We believe that there is a steadily diminishing confidence amongst the more enlightened practitioners in arsenic as the remedy for every cutaneous ailment. It is too long over its work, even when it does cure, which is by no means always certain.]

To give a practical turn to this note—for I do not dignify it by the title of "paper"—let me refer to one important principle of treatment to which I think we do great violence in our treatment of skin complaints—the principle of using soothing remedies locally to all conditions and stages of cutaneous maladies in which *active* congestion is a feature.

By a soothing treatment, I mean such as prevents congestion, and secures the exclusion of air from the diseased part. The access of oxygen is the great accelerator of changes in the inflammatory stages of skin diseases, especially where the cuticle is more or less destroyed or absent. The remedies that most effectually soothe are baths of various acids, astringents and powders, and simple non-rancid unguents to which astringents are added. Sometimes fatty substances rather increase the irritation, especially if at all rancid. Now what is this kind of treatment, especially for the exclusion of air, but the securing of *rest* for the skin. In proportion as our knowledge of the pathology of disease increases, we learn the definite influence of external conditions and agencies in operation upon the skin in a diseased state, and acts and agencies which we thought of little moment, or even insignificant for or against the cure, appear to us, in the progress of our better knowledge, of great importance. It is sometimes difficult to bring ourselves to believe that simples are most efficacious; the very simplicity makes us attach little importance to these simples. We are very apt at the mention of them to say—"Oh, yes, I know that of course," but to neglect, nevertheless, what we feel without hesitation is so self-evident on reflection. These simple things, indeed, are overlooked in practice. In the early stage of a disease which general experience has taught us will very likely become chronic, and, it may be, pertinaciously obstinate, the natural idea and desire is, cut it short, if possible, by heroic measures. I do believe that this feeling leads us to do more harm than good in the early congestive stages of many skin diseases.

Now a simple soothing plan of treatment, therefore, acts in two ways:—First, negatively, in so far that it takes the place of the use of more powerful remedies that would increase congestion; and secondly, that it directly tends, of course, to diminish congestion. This suggests the question—What relation has congestion to disease?

Take urticaria: here it is secondary to the loss of tonicity consequent upon perverted innervation. Take psoriasis: it is now taught that this disease is merely an overgrowth of the cells of the rete, due to hyperæmia and statis in the capillaries of the papillary layer of the skin. Take lupus: it is the consequence of the demand made upon the circulation for sustenance, by a rapid growth of entirely new granulation tissue. In the first, congestion is of little importance *per se;* in the second case (psoriasis), at the outset, it is the most important point to be alluded to; for if we can cut off the supplies, the hypertrophic growth of cells cannot take place; and in the third disease (lupus), secondary though it be, to the real mischief, yet it may, under certain circumstances, be a possibility, where the disease for instance is just beginning, to check the congestion, and thereby starve the disease into submission. It is in the earliest stages, before the deep vessels are seriously involved, and when the congestion is impressionable, that our soothing remedies do great good, and it is also at this time, and under these circumstances, that the use of irritants is sure to do harm. So fully am I persuaded of this from clinical observation, that one of the principles of treatment which I most dogmatically teach to students with, it may be, tedious reiteration, is this:—"Wherever and whenever, you meet with a hot, red, tender, and irritable skin complaint, be sure you do not apply any irritant, but invariably soothe."

I will mention diseases in which the adoption of this principle of treatment is often accompanied by eminent success:—Acute general psoriasis of the young; erythema and acne of the face; erythematous lupus; lichen ruber; eczema, and syphilitic roseola.

*Acute General Psoriasis.*—Some of the best results I have ever seen in the treatment of this form of disease have been in young children recently under my care in University College Hospital. I found that the application of tarry applications have considerably increased the congestion of the skin; the disease has consequently spread, and I am sure been prolonged; but with alkaline and bran baths each night, with subsequent oiling, and prescribing cod-liver oil and good living, I have soon put my patients into good condition again. This is my plan in future for all cases of acute general psoriasis where the skin is much congested.

In regard to the use of tarry preparation in psoriasis in general, I am convinced that when congestion is at all well marked it should be used with caution, and if it irritate I prefer the use of wet packing for a little time before recurring to its use again.

*Erythema of the Face and Acne.*—Very recently we have been pitting different plans of treating acne and erythema of the face, one against the other, amongst my patients at University College Hospital. It so happens that a good many American physicians have been to see our practice after visiting Hebra's Clinique at Vienna, and have as usual eulogised the soap treatment in vogue there. Well, we have tried it, but I will not try it again —for all my cases have got worse wherever there has been any marked congestion. In cases of torpid action of the skin of the face generally, unquestionably it rouses the sluggish follicles into activity, and helps the cure of the disease, but it then acts in the same way as do the many compounds of sulphur in repute; but in the early stages, and where the face flushes actively after meals or exposure, soothing remedies, and none so good as a lotion made with about half an ounce of pale prepared calamine powder—frequently applied after hot bathing, with suitable internal remedies to meet dyspepsia, pyrosis, and uterine troubles—never disappoint it seems to me; but stimulants applied externally, whilst they now and again act happily in the early stages, very oftentimes extend the congestion much beyond the original area and degree. I am not speaking of course of the chronic stages of disease.

*Lupus.*—I speak especially of erythematous lupus. Clinically, the results of employing some mild lotion containing an impalpable slight astringent powder, are very satisfactory. This is my favourite plan of treatment, in connexion with suitable internal remedies, for anæmia, sparcity of fat, lymphatic or strumous temperament, under-feeding, and so-on. The most suitable cases are those in which the disease is so thin that the pressure of the finger occasions the congestion to disappear, and nearly as it were obliterates the disease. There may be specks, or almost tubercles, here and there formed by the new granulation tissues, which can be destroyed by caustic potash in solution, the use of the soothing remedy not in the least being interfered with. Given a mass of new lupus tissue and much congestion, the certainty is that the former will spread if irritants are applied; lessen the surrounding congestion, and the disease will, with perhaps one application of caustic, rapidly go. I speak from actual cases recently under my care. The object is to check the congestion, and so starve the disease or cell change that induces the congestion.

*Lichen Ruber.*—Is a very rare disease in England. I have

recently seen two cases of the disease which illustrate my point so well that I cannot help referring to them. This disease is characterised essentially by the development of solid *red papules* (lichen ruber), caused by the effusion of lymph about the hair follicles. In one of the cases I have had no difficulty of seeing the early stage of this disease, and it commenced by rapid engorgement of the follicular plexuses; it seems to me to be dependent upon disorder of the sympathetic nervous system. The skin is generally red, and "burns" intensely, the congestion being increased by excitement and worry and dyspepsia. In fact, the capillary congestion seems to be quite in character, like that of blushing. Now I know, as the result of careful experiment, that it is very easy to aggravate the congestion in lichen ruber. Too hot a bath will do it—the heat of the bed—warm drinks—a cold wind; sleeplessness, with the mental irritation attending it, will do so. Even arsenic, which is said by Hebra to be *the* remedy for the disease, if pushed far enough, will certainly, as in one of my cases, increase the irritability when its semi-poisonous operation fairly commences.

The exclusion of air by a semi-paste made of zinc, glycerine, and liquor plumbi I found very efficacious, and when arsenic failed my case got well with that external application, and the free exhibition internally of assafœtida.

*Eczema.*—Without entering into the intimate pathology of eczema, I may be permitted to say that it seems very clear that there is no special blood state or diathesis upon which eczema depends, but that it results from, in the first place, perverted innervation—this being capable of directly inducing, not only vascular changes, but even changes in the cell elements of the skin. The grounds upon which this view of eczema is taken, and on which I base the assertion that eczema is a catarrhal inflammation of the skin, have recently been discussed in the Lettsomian lectures, which I delivered before the Medical Society of London this session. My point now is, that the application of soothing remedies acts in eczema in the early congestive stages in two ways.

It allays irritation of nerves directly—hence the value of applying poultices and fomentation; and secondly, it diminishes the congestion, which favours the "discharge" of eczema. The vesiculating process breaks away the cuticle, leaves the rete and it may be the derma exposed, and favours the free access of oxygen. The glycerine plasma and oxide of zinc and starch powders and pastes act under such circumstances with the best effects.

I might refer to other diseases, but I think I have said enough to indicate the value of following the very simple principle of treatment under discussion. I hope it will not be thought that

I am at all inclined in any way to side with those who advocate "the expectant treatment of disease," as far as, for instance, to the exhibition of mint water in acute rheumatism. I am scarcely so cruel as that. I yield to none in the freedom and faith with which I use even potent drugs to skin mischiefs after their congestive stages have passed or succumbed to our remedies. Nor do I think the use of soothing remedies at all part of a do-nothing system. Negatively, it prevents much harm, and positively lessens tissue changes, through its control over congestion.

One more point I am anxious to call attention to touching the influence of active congestion in skin diseases. It is of little use to treat the congestion itself when the cause of the congestion is exceptional, such as the free circulation of excreta. This is a very active cause of intensification of congestive phenomena. It constantly happens that soothing remedies fail where we should expect them to act satisfactorily. The reason is that we overlook conditions of the fluid circulating through the capillaries of the diseased part, which do neither more nor less than "irritate" from within as much as cold or acrid substances do from without. The retention of excreta from deficient kidney action, or liver action, and gout, at once occur to some here. We speak of gouty eczema, gouty psoriasis, gouty lichen. To our minds these expressions mean that gout causes the eczema, the psoriasis, the lichen, as though it could be the *vera causa* of pathological processes the most dissimilar. To me they mean that these diseases may occur in gouty subjects, and are intensified, aggravated by the circulation of retained excreta, and that the treatment should be adapted to remove the exceptional cause of irritation,—which will ensure chronicity, if not removed,—in conjunction with the remedies appropriate to the eczema, psoriasis, or the lichen, &c.—*Journal of Cutaneous Medicine, Dec.*, 1870, *p.* 166.

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## 68.—REMARKS ON THE RATIONALE OF THE ACTION OF ARSENIC ON CUTANEOUS AND OTHER DISEASES.

By Dr. JOHN CLELAND, Professor of Anatomy and Physiology, Queen's College, Galway.

[The cases in which Dr. Cleland has seen the greatest advantage from the use of arsenic have been mostly of a description in which its virtues have been very generally admitted, such as cases of eczema and psoriasis in patients of a gouty or rheumatic diathesis; cases of psoriasis in conjunction with uterine affections; chronic inflammatory uterine affections even though unaccompanied with cutaneous disease; persistent rheumatic ophthalmia, and cases of lupus and palatal ulceration.]

If we review the physiological actions of arsenic, in the hope of their shedding some light on its therapeutic properties, we probably cannot have a better guide than the Memoir of M. Lolliot. From experiments on dogs and rabbits, M. Lolliot concludes that "arsenic exhibits two fundamental properties: first, depression of temperature; secondly, diminution of the urea in the urine." He proceeds then to say, that "it is with justice that M. Sée has now ranged arsenic among the *medicaments d'epargne*, besides alcohol, coffee, &c. Arsenic, according to M. Sée, owes this property to direct combination with the blood globules. This combination, according to him, is made at the expense of the oxygen, of which arsenic takes the place, —and the globules, deprived of their oxygen, are no longer fit to oxidise our tissues, and the denutrition of these is thus lessened. In fact, in the bodies of persons poisoned with arsenic it has been observed that the globules are conserved—that is, that they are preserved from decomposition in consequence of the want of oxygen. Arsenic prevents then the globules from being charged with oxygen in the lungs, and so renders them incapable of oxidising the tissues; from this arrest of combustion results fatty metamorphosis of the organs." M. Lolliot is unable to find by experiments on animals any effect produced by arsenic on the respiration, and imputes the "singular property which arsenic possesses of rendering the respiration easier," which he considers demonstrated by observations on arsenic eaters, to the reduced denutrition of the tissues. So also in arsenicism, or slow poisoning with arsenic, the motor paralysis and somnolence produced, being accompanied, as they usually are, with emaciation, may, he thinks, be reasonably imputed to the imperfect nutrition of the brain and cord. He finds that arsenic is eliminated by the skin, mucous membranes, kidneys, and liver, and in large doses produces fatty degeneration of the liver and kidneys. From this account, even keeping the theoretical part of it out of view, it appears pretty plainly that it is unlikely that the therapeutic benefits of arsenic are due to elimination by promotion of the ordinary action of the emunctories. Particularly, it is unlikely that it promotes the elimination of a *materies morbi* by means of the kidney, when experiment demonstrates that it acts with certainty as a diminisher of the total amount of urea secreted in twenty-four hours. No doubt that diminution of urea depends most probably not on action on the kidney, but on the material from which urea is formed, and it is quite possible that, although by that action the work thrown on the kidneys is diminished, the kidneys may yet have their efficiency increased in accomplishing the work which is left to them; and this seems the more probable, as the very irritation which in excess produces fatty degeneration

might be expected when more moderate to stimulate the proper function. But it is noticeable that while arsenic diminishes the urea it does not seem to increase the other ingredients of the urine, even though it is itself eliminated, at least in part, by the kidney. Neither is its action due to increase of the amount of intestinal secretion, for in those instances in which it is most efficacious there is no diarrhœa whatever, and if that symptom should threaten, it is necessary immediately to take the hint and discontinue the exhibition of the remedy. Neither are the secretions of the skin increased so as to constitute arsenic a sudorific; and yet on integumentary surfaces a certain action is exercised of a very definite nature, as shown by three usual effects of medicinal doses,—the silvery tongue, plumpness of the face, and irritation of the conjunctiva. Of these, the silvery tongue described by Begbie is generally the first to appear, and I think I may say that I have never seen any therapeutic effect produced without the tongue having assumed the silvery appearance—an appearance due altogether to a change in the epithelium, probably to thickening of it, but certainly not produced by adherent deposit. The plumpness of the features, so far from arising from any œdematous condition, looks perfectly healthy, and is best marked when the remedy agrees with the patient. But the irritation of the conjunctiva is often absent, or is unobserved by the patient, although the therapeutic advantage sought has been gained. When it is present it is not a condition marked by increased secretion, but redness and a certain soreness, or "prickliness." I take it that all the three symptoms,—the silvery tongue, the plump appearance of the face, and the red conjunctiva,—are indications of one state of matters, injection of the superficial capillaries supplying nourishment to a rapid growth of epithelium.

We have now, I think, brought before us two very marked sets of physiological properties of arsenic in medicinal doses, not obviously connected one with the other: first, the arrest of processes of decomposition, as evinced in experiments on animals by diminished secretion of urea and reduction of temperature; secondly, increased nutrition of epithelial surfaces, as evinced by the phenomena we have just considered. The second set of properties might of themselves be supposed to furnish some explanation of the therapeutic benefits in cutaneous diseases, but it is on the first set of properties that we must fall back for an explanation of the benefits derived in rheumatic affections; and considering that the cutaneous affections are often connected with rheumatic habit, the beneficial effects on these are probably also derived in some measure from the first set of properties.

I have stated already why I think that the groups of affec-

tions associated with rheumatism and gout have probably all of them this in common, that the tissues are acted on by a poison in the blood, a product of denutrition, and that the poison is different in different sets of cases. I would further add that the group probably is divisible into two very different sets of cases: first, there are those in which only the normal amount of debris is thrown into the blood, but it fails to be sufficiently rapidly removed by the emunctories, and the question might be raised with regard to those, whether the emunctories are to blame or the debris of tissue has an abnormally slight tendency to decomposition; secondly, there are those in which debris of tissue is thrown too rapidly into the blood, from deficient elaboration of the substance of the tissues, rendering them prone to rapid decay; and possibly a third set of cases might be distinguished in which the emunctories have too much work thrown on them by overabundant new supplies of material being constantly thrown into the blood. In lithic diathesis, there can surely be little doubt of the truth of the distinctions now drawn: typical gout belongs to the first division now made, and I have no hesitation in believing that the cases marked by lithic diathesis in which arsenic does good belong to the second group. If this be the case, and we consider that arsenic circulating in the blood has the property of preventing processes of decomposition, we have a ready explanation of its benefits in those cases.

The circumstance that creosote and kindred antiseptic substances are of much use in some rheumatic affections would corroborate this view of the mode in which arsenic is beneficial, and so also would the consideration that those cutaneous diseases in which arsenic does good are those in which tarry and such like applications are of use. Tar in psoriasis, although applied to the surface, certainly must exert its influence, not on the superficial, but on the young strata of epithelial cells,—for it is only by a change in the young strata that the diseased superficial strata of epithelium can be replaced by healthy; and it is quite possible that the influence which it exerts on the young epithelium may consist in prevention of a too easy oxidation—in fact be of an antiseptic character. But it may be questioned if any remedy combines like arsenic the property of preventing waste of tissue with that of fostering growth of epithelial textures.—*Journal of Cutaneous Medicine, Dec.* 1870, *p.* 190.

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## 69.—ON THE TRANSPLANTATION OF SKIN.

By CHARLES STEELE, Esq., Surgeon to the Bristol Royal Infirmary.

[The vital power of portions of transplanted skin has been repeatedly proved in cases of total detachments, as well as in

the remarkable results of plastic operations. It appears that Dr. Frank Hamilton, of New York, first suggested this plan as early as 1847, but put it into operation for the first time in 1854, in the case of a man who had lost a large portion of integument of his leg by an accident. He did not, however, discover that the graft might be wholly separated before insertion. M. Reverdin demonstrated only recently that the portions of skin might be taken from any part of the body.]

M. Reverdin's paper on Epidermic Grafting was read before the Surgical Society of Paris in December of last year, and published in the *Bulletin* of the Society for that year, and also in the *Gazette des Hôpitaux* for January 11th and 22nd, 1870. Mr. George Pollock heard of M. Reverdin's experiments in May last, and immediately tried the treatment on a most suitable case, in a child eight years of age, who had been in St. George's Hospital for three months and a half with a very extensive burn of the right thigh, of more than two years' duration. The ulcerated surface extended from the buttock to the knee; it was broad above, narrow and pointed below. The progress and success of this case attracted considerable attention, and the practice was soon adopted in most of the London and several provincial hospitals.

In applying this treatment to ulcers, we must bear in mind the two leading objects of treatment—firstly, rapidity of cure; and secondly, permanence of cicatrisation. The following points are of importance to consider when we are about to operate: the state of the ulcer; the size and number of pieces to be grafted; how near they should be placed; whether the whole or only part of the cutis should be inserted.

It is necessary that the granulating surface be in a healthy or tolerably healthy condition. This was insisted on by Mr. Pollock and Mr. Lawson in an interesting discussion at the Clinical Society on November 11th, and has been felt to be an essential by all who have practised the operation; and, where failure has followed skin-grafting, an ill-conditioned state of ulcer has generally been the cause. I feel that, where there is any marginal cicatrisation, or disposition to form the same, grafting may safely be practised, and the process from the margin will rapidly advance.

With regard to the size of pieces of skin and the depth of true skin to be used, it has been clearly shown that those employed by M. Reverdin and Mr. Pollock and many others, of the size of a millet-seed or an oat, whether including the whole or only part of the cutis vera, answer admirably; while the minutest subdivisions, as carried out by Mr. Dobson at the Bristol General Hospital in some striking cases which he brought before a meeting of the Bath and Bristol Branch of the Associa-

tion on October 27th, set up rapid cicatrisation; and portions of entire skin a quarter of an inch in diameter also answer well. From these facts, it is evident that all that is essential is the papillary layer of cutis, no matter how small, capable of developing cuticle, and therefore cicatrisation. But, in dealing with individual cases, I feel it right to bear in mind the size of cicatrix which will result, and the strain to which it will be subjected. While, therefore, on the grounds of economy of skin and consideration for the patient, we should use little skin in such ulcers as will not be subjected to great strain, yet in those which will, as large ulcers on the leg, which, after being healed, frequently disappoint by breaking out again, it is most evident that to transplant a number of fair-sized portions of entire skin, which will retain certainly their elasticity, and so, by their individual stretching, relieve the tension of the whole surface, is the most likely to be permanently successful. Transplanted skin does not retain its perfect integrity and function, as does skin employed in Taliacotian operations; since, in the first place, we cut it so close to its under-surface, in order to avoid fat and areolar tissue, that we must cut through most, if not all, the sweat glands and hair-bulbs; and, secondly, the new piece sheds its epidermis, and for a time takes on the appearance of granulations, and, when cicatrisation has taken place, has more the smooth look of a cicatrix than the furrowed and soft appearance of true skin. Yet it contains its elastic tissue, is thicker in consistence, and has a more natural feeling and appearance than the cicatrix formed around it. Experiments have shown how little may be used with good results; they will yet show, I feel sure, that far larger portions may be employed than have yet been, and with great advantage, as, for instance, in relieving contractions from burns. I have now under treatment a case of this nature, in which I expect to show that a piece of the size of a penny will succeed, and give elasticity in a situation where it is greatly needed—namely, in the bend of the elbow. It is clear that, the greater the number of centres, the more rapid the cure must be, and the firmer the cicatrix; but I am inclined to think that, in a large surface, the same sized piece of skin would prove more useful if employed in a series of graftings than if distributed in exactly the same spots at one operation; for I am convinced that each series gives fresh life and vigour to the whole surface. For the same reason, since each graft is found capable of developing a cicatrix from a quarter of an inch to an inch in diameter, it is wise to insert the portions of skin from an inch to two inches apart, and subsequently place fresh points between these.

In operating, a portion of skin is pinched up in a forceps, or between the finger and thumb, and removed, either in the en-

tire thickness or in part: it is essential on the one hand that no areolar tissue and fat, on the other that the papillary layer of cutis, be removed. The granulations, if quite healthy, need only be clean; if not quite bright and active, they are slightly incised or scratched; and, when bleeding has stopped, the graft is laid upon the surface. The portion removed is easily cut up, if desired, on the thumb-nail, and each portion can well be applied with the point of the scalpel. Some use a narrow strip of ordinary plaster, some transparent isinglass plaster, others Lister's lac plaster. I have used these, and also a strip of gutta-percha tissue, which holds the portions of skin firmly in place, and, being transparent, enables one to see, while stretching it across, that the grafts do not slip. Over these, strapping, water-dressing, or any lotion suitable to the state of the granulations, should be applied; then a compress of cotton-wool, retained by a bandage, rather firmly applied, to insure close adaptation of the grafts to the granulations; the wool also serves to keep the grafts warm. I feel it wise to avoid ointments in the early dressings, as particles of grease might insinuate themselves beneath the transplantations and separate them. Unless there be copious suppuration, it is well not to disturb the dressings till the second day; the appearance then presented is the epidermis of the graft lying free on the granulation dressings, or on the graft; if spread out, it shows the original size of the graft, which now is contracted and pale. During the next few days, the graft becomes vascular, and looks very like the surrounding granulations, and is nearly lost to view unless it be of some size, when it appears as a raised mass. It is difficult, and sometimes impossible, for even the operator to distinguish the grafts, if small, for the first seven to twelve days. The first indication of activity is a faint blue cicatrising aspect in the site of the graft, and I also observe, if the graft be near the circumference, a line of cicatrisation running from the circumference to the graft, and one from the graft to the circumference, which unite and increase to a band, and so increase again the cicatrix-producing margin; in fact, these lines shoot out in various directions to any near margin or points, and thus in time may divide an ulcer into a series of smaller ones. One fact of great importance and value, and which shows the remarkable influence of the transplantations on the whole ulcer, is the rapidity with which cicatrisation from the margin proceeds directly the grafts have established themselves, and even before they show much cicatrix of their own. Mr. Couper observed the same fact in a case under his care in the London Hospital, but remarked that he was not prepared to consider it more than a remarkable coincidence. I however, feel it to be one of the most valuable immediate effects

of transplantation, that the grafts act as natural stimulants to the ulcer generally, and arouse renewed energy in the marginal cicatrising edge, wearied with its previous almost hopeless exertion.

Another field of usefulness is open to skin-grafting in cases of severe lacerated wounds needing partial amputation, or involving considerable sloughing; where the prospect of being able to transplant when granulation is fairly established, will enable us to save more of valuable parts than we should otherwise have ventured to leave uncovered by any skin, trusting alone to granulation. Such a case was recently admitted into the Bristol Infirmary. A man had his fingers and hand severely crushed and lacerated by cog-wheels, the skin in rags, the muscle mashed, and the metacarpal bones comminuted. The thumb was uninjured, and I amputated through the middle of the palm, pinching off the bones near the carpo-metacarpal joints. There was no skin to form a covering, and some sloughing of the soft parts has followed; but granulation has commenced, and, when it is fairly established, by transplanting on to the palm, and the surface covering the ends of the bones, I shall be able to materially hasten recovery and give a firmer, harder, more yielding substance for use. In retraction of stumps leaving bones covered with granulations only, transplantation of skin will be of great service, and will doubtless save some secondary amputations. Finally, following the example of Mr. Durham, of Guy's Hospital, by modification of skin-grafting, we shall be enabled to remove, with boldness, tumours involving the loss of much integument.

Another great benefit to be derived from transplantation is that of arresting the breaking down of large cicatrices. This has just been illustrated in the case of J. Dando, already mentioned. On November 30th, an ulcer formed in a large healed surface of thin cicatrix midway between the sound skin and the grafts. On December 2nd, I planted two portions of skin of the size of a pea upon this oblong surface. One, the lower, lived, and, on December 6th, was becoming red; and had stimulated fine blue cicatrisation from the margin to cover half its surrounding ulcer; while the upper had died and ulcerated the surface on which it was planted, and was increased to fully a third larger. On this, and also on another ulcer which had formed, I planted a piece of skin of the size of a fourpenny-piece.

With regard to the source from which integument should be taken, most prefer the inner side of the upper arm; but one important feature of the operation is that it gives freedom of removal from any part or any person, and our endeavour should be to gain integument of the same consistence as that belonging

to the part on which we plant it, while we are careful not to produce tension in the part from which we remove skin. But we are not confined to our patient even; an amputation giving healthy skin affords an abundant supply. Mr. Leonard and I both made use lately of this source from a leg amputated by Mr. Tibbits. Mr. Leonard's grafts took, but mine failed, owing to the unhealthy character of the ulcer—a syphilo-strumous ulcer covered with aplastic lymph. I was anxious to try skin from such a source, and scraped the lymph from several granulations; though the soil was unsuitable, some grafts adhered for a time, and gave assurance that they would have succeeded in a healthy ulcer. I have since employed healthy skin from an amputated limb in three cases.—*British Med. Journal, Dec.* 10, 1870, *p.* 621.

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## 70.—ON THE TRUE NATURE OF THE SO-CALLED SKIN-GRAFTING.

By Dr. DAVID PAGE, President of the Royal Medical Society of Edinburgh.

I am convinced that the so-called "skin-grafting" consists, in truth, not of a transplantation of true skin, but of epithelium, exactly analogous to what is occasionally observed in certain ulcers, especially in those associated with a syphilitic cachexia, where patches of delicate cuticle appear spontaneously on the surface and spread towards the circumference, but to be carefully discriminated from similar phenomena in ulcers resulting from extensive burns, for in these the apparent regeneration of skin is due to the fact that little insulated spots of true skin having escaped destruction, reappear after the first effects of the injury have been removed. These act as centres for healing action in a very different way. That the action of the transplanted portions is not merely that of affording a point of attachment (*point a' appui*) for spontaneous cicatrisation, was satisfactorily proved by a series of experiments, in which pieces of sheep's-skin, caoutchouc, and other substances, were applied to the surfaces of healthy ulcers, and then retained in the same way as the portions of skin. But they only acted as foreign bodies, and on removal I found that a destruction of the granulations and depression of surface had resulted from their pressure.

Besides the cases in which skin-grafting has proved successful, have been those in which the granulating action was healthy; for if it were weak or absent, the patches refused to adhere. The temporary disappearance of some of the transplanted pieces, noticed by at least one surgeon, is, I think, due to the varying thickness of the young epithelial layer removed.

In conclusion, I am led to believe by these results of observation that beyond a somewhat greater rapidity of cicatrisation, especially where contraction of surrounding textures is resisted either by the situation or the extent of the breach of the surface, this method of skin-grafting is of limited application; and as its results lead to no regeneration of the true skin, but merely the formation of a cicatrix in every respect identical with that formed spontaneously in the natural process of cure. It is an operation which cannot rank with plastic operations proper, and which is not likely to occupy a permanent position in minor surgery.—*British Med. Journal, Dec.* 17, 1870, *p.* 655.

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## 71.—SKIN-GRAFTING.

By R. W. GOLDIE, Esq., Assistant Surgeon, Chorlton Union Hospitals, Manchester.

Eight out of ten of the ulcers met with in every-day life, when subjected to the most ordinary remedies, rapidly get well. Of these remedies I may instance the following as among the chief—viz., rest, cleanliness, and caustic. The ninth ulcer will require for its cure appliances of a more powerful description, such as strong nitric acid, strapping skilfully and carefully applied, &c., &c., in addition to internal remedies and diet suited to the constitutional requirements and general condition of the patient.

The tenth ulcer—of which class there are, happily, few—will resist every known method of treatment, although persevered with for a great number of years, under the most favourable circumstances alike for surgeon and patient. It is for cases of this last description that skin-grafting promises at present to do so much, and it is because I have under my care at the present time an ulcer whose duration, position on leg, and antecedent treatment fully warrant me in calling it an *incurable ulcer*, that I venture to submit the following particulars of the case.

Lawrence D., aged fifty-five, was admitted Oct. 15th, having an ulcer, somewhat club-shaped, situated in the front and lower part of the leg, eight inches long and ranging from two to four inches in breadth. He states that he injured his leg by falling against the sharp edge of a bucket twenty-eight years ago. He was treated for this and recovered; but eight years afterwards—i.e., in 1850—he again injured his leg in the same situation, since which time, although he has been treated in many institutions, including this one, he has received but little benefit up to the present. With such a history I felt it would be but idle waste of time to try any of the usual remedies in this case,

and accordingly set about procuring a healthy surface for the purpose of grafting.

On October 22nd I transplanted from the skin of the back of the upper arm three small pieces, each the size of a pea, and bound them to the granulating surface with ordinary soap plaster. Ordered water dressing during the day, and zinc ointment at night, to be applied to the uncovered portions of the ulcer, with a bandage over all.

25th. On removing the plaster, I found all three adhering; the bits of skin no longer looked white, but were of a reddish-blue colour, and elevated in the centre. Ulcer dressed as before.

In this, as in some of my other cases, no change took place for seven or eight days—the portions remaining perfectly visible in all my cases during this period—when they rapidly began to increase in circumference, the margin of the ulcer also closing in with almost equal rapidity.

At the time of writing (16th), the two lower grafts are completely merged in the surrounding cicatrix; the upper one has increased to the size of a florin, and is united in one part to the surrounding new skin; the remainder of the circumference and the margin of the ulcer are almost united.

I may here state that I have of late abandoned the use of scissors, and now employ dressing forceps and bistoury in procuring the grafts. The bistoury cuts more quickly and accurately, therefore less painfully. In my first case I bound the graft on with collodion; this proved useless, as graft and collodion were floated off by the discharge in twenty-four hours.

In conclusion, the value of Reverdin's discovery does not alone lie in enabling the surgeon to cure what have hitherto proved incurable ulcers, but also in curing them with unprecedented speed.—*Lancet, Jan.* 14, 1871, *p.* 46.

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## 72.—ON THE TREATMENT OF ECZEMA.

By ERASMUS WILSON, Esq., F.R.S., Professor of Dermatology at the Royal College of Surgeons of England.

In reference to treatment, eczema must be regarded as a "solution of continuity," and as such requires a dressing to defend the lesion, for the purpose of keeping its broken edges in contact, and of excluding the operation of external irritants of every kind. A dressing with the benzoated ointment of oxide of zinc fulfils this indication completely. The ointment should, in the first instance, be smeared upon the eczema; the smearing process should be repeated twice or more frequently in the day, so as to secure a permanent covering; and, wherever

practicable, strips of linen rag spread with the same ointment may be placed over the eruption and maintained in position by a roller, so as to keep the ointment and the dressing in permanent contact with the diseased skin. In this relation the ointment fulfils the purpose of a light unirritating plaster; it preserves the natural moisture of the skin, and excludes the stimulant operation of the atmospheric air. It is easily replaced when accidentally removed, and in many cases will accomplish every object of treatment from the beginning to the cure of the case. Let me instance an eczema infantile, in which the body is covered with eruption from head to foot; it is hardly possible to conceive a remedy more admirably suited in such a case to meet the exigencies of treatment than the benzoated ointment of oxide of zinc.

It has long been my custom to combine with the zinc-ointment a small quantity of spirits of wine, in the proportion of one drachm to the ounce: the spirit softens the ointment and facilitates its application; it produces a sensation of coolness which is agreeable to the heated surface, and it has besides a gently stimulant effect on the nerves of the skin. The application, as I have already observed, relieves the heat, the stiffness, and the itching, and the relief continues until the ointment dries up or is accidentally removed. When either of these events takes place, the ointment must be repeated in the same manner as before. Our aim should be to favour a thin deposit or concretion on the surface, which should occupy the place of the broken or exorciated epidermis; such a deposit or concretion is, in fact, an artificial cuticle, a kind of natural plaster, under which the skin is preserved in a state of repose, and the inflamed and irritable tissues have time given them to recover their normal status. You will perceive that eczema brings before us a surgical lesion in which the application of a plaster or dressing of any kind is often perfectly impracticable, and therefore we are obliged to have recourse to a remedy which shall possess the intrinsic property of adhesion to the skin without the aid of other coverings. This will explain our anxiety to have the inflamed surface thoroughly coated with the ointment, and to have the ointment repeated as often as, from any accident, it may by chance be displaced; and, of course, wherever practicable, means may be contrived, such as bandages and compresses, to render the dressing permanent. Above all, it is important to avoid washing the skin at this stage of the disease: washing would remove the ointment and open up the excoriation afresh; it would be undoing that which it had taken much labour to do; it would be unsurgical, by creating disturbance where rest and position were of the utmost importance. There can be no want of cleanliness where a pure oxide of zinc oint-

ment is properly used; and if secretions be poured out from the skin, they may always be absorbed by means of a soft napkin, and, together with crusts, may be gently wiped away. Mothers will sometimes hear with wonder the command to avoid washing their children in a case of eczema infantile; but they are generally ready to admit that the eruption is always more uneasy after the washing than it was before, and their instinct quickly assures them that their instructions to avoid washing are correct.

Eczema enjoys the peculiarity of presenting, in a general attack of the eruption, a variety of degrees or forms of manifestation on different parts of the body; thus it may be erythematous in one place, ichorous in a second, and squamous in a third. The zinc-ointment is equal to the relief of every one of the forms of the eruption; but it not unfrequently happens that we may find an advantage in using a powder in one situation, the lime-water and zinc-lotion in a second, and the ointment in a third. And in the adoption of these means we are to be influenced rather by the sensations of the patient than by any preconceived theory of the disease. In general terms, it may be said that whatever gives rise to pain or uneasiness is bad, and will require to be changed.

Eczema is essentially chronic as to its course; but it manifests stages in its progress which are as decidedly acute. The treatment which we have just been considering is strictly palliative, and is therefore especially adapted for the early and acute stages and acute manifestations of the disease, but is equally applicable for the relief of heat, itching, and dryness, in every stage of the affection. If I were asked how I should distinguish between an acute and a chronic eczema, I should say that the acute stage was denoted by erythematous congestion and moist excoriation, and the chronic stage by infiltration and desquamation, the infiltration giving rise to thickening and induration. I shall now suppose that the eczema has reached its dry, squamous, thickened, and indurated stage. It has assumed that condition which, in ulcers, for example, is termed indolent, inveterate and so forth. The zinc-ointment removes the scales, heals chaps and fissures, and relieves in some degree the itching, but does nothing towards the dispersion of the thickening and induration of the tissues—that is, towards the cure of the disease. The time, in fact, has come when we must contrive to awaken the tissues from their indolence and lethargy, and direct, as far as we are able, the newly awakened life towards a normal state of action—that is, towards a restoration of healthy function and tone.

Our purpose, therefore, is no longer palliative; it is stimulant or tonic; the palliative treatment has accomplished all that it

can; now we have to consider the nature and mode of application of the stimulant treatment. Our stimulants possess a great range of power, and, if we knew exactly the amount of resistance which we should meet, we might apportion our power with exact accuracy. But as this can never be the case in dealing with so delicate a machine as living organisation, the laws of surgery would prescribe to us that we should begin with the mildest of our stimulants and advance progressively and to the best of our judgment to the higher degrees. The philosophic surgeon will not hesitate to adopt this course as one in every way consistent with his knowledge of vital phenomena; and, therefore, we may proceed to inquire, In what do our stimulants consist?

The first and best of our tissue-tonics or local stimulant remedies is undoubtedly soap. It will be remembered that I have prohibited soap in the earlier and acute stages of the eruption; therefore soap is new to the morbid skin, and capable, consequently, of producing a more powerful effect than would otherwise be the case. The chronic eczema should be thoroughly washed with soap, combining with the solvent action of the soap upon the epithelium such an amount of moderate friction and compression as may reach the vascular and the infiltrated tissues. After the washing, the surface is to be dried with a soft napkin, and, as soon as dried, dressed with the zinc-ointment like an eczema of an earlier period. The next day we examine very carefully our eruption, and, if we find no signs of excessive irritation present, we renew the saponaceous washing, and repeat it again and again until the eczema is healed. It will soon be apparent how frequently the process may be repeated, whether once a day only or twice, or whether the reapplication must be deferred for several days. The immediate consequence of the stimulant treatment will probably be the development of an exudation on the surface, which will relieve the infiltration and turgesence within; and a few repetitions of this exudation will so far tend to empty the tissues of their excess of fluids that they will be enabled to return by degrees to their normal state.

This, in fact, is the explanation of the *modus operandi* of the powerful stimulants which we hear of as being from time to time useful in the cure of chronic eczema; such as strong alkaline soaps, strong solutions of potash, strong mercurial ointments, strong solutions of nitrate of silver and tar. And, in illustration of the same principle, I may call your attention to a remark which I made in reference to the treatment of No. 27; namely, that, after an obstinate resistance of a variety of remedies, the disease was suddenly cured by one application of the tincture of croton, pencilled over the inflamed surface.

If I wished to reduce the principle of local treatment to an aphorism, I might do so by the use of the two words *palliative* and *stimulant*. Among the palliatives, there is one which is occasionally of much service for the relief of heat, tension, and pruritus; namely, water-dressing, whether the moist application be made in the ordinary way with an impermeable covering, or employed in the shape of a cold starch-poultice. The water-dressing may be used constantly for a period, or during the night only; and its use may be combined with that of the lime-water lotion, zinc-ointment, or saponaceous frictions and ablutions. The water-dressing is very useful in accelerating the separation and removal of crusts, in promoting exudation from infiltrated and œdematous tissues, and in preparing the tender skin for the treatment by ointment or the treatment by desiccating lotions. If a water-dressing had been kept applied to the leg in Case 47 for twenty-four hours, the whole of the dark crust visible in the model might have been washed away. The morbid skin would then have been prepared to receive the dressing of zinc-ointment; and this, with the support and moderate pressure of a properly adjusted roller, would have effected a vast improvement in the disease in a very short space of time.

One symptom which more than any other torments both the surgeon and the patient in eczema is *pruritus;* and very frequently we are called upon to change our plan of treatment in order to combat this annoyance. When the pruritus proceeds from ordinary irritation, it may be subdued by the oxide of zinc-ointment; and a moderate friction with this ointment may be had recourse to as often as the itching returns. If allowed to continue, the pruritus will sometimes assume a neurotic and neuralgic character. It will come on after any slight exertion, after the taking of food, upon changes of temperature, and especially at night, sometimes on first assuming the reclining posture, and at other times in the early morning. It is always intermittent, and presents such a variety of manifestation as to make evident that sometimes the peripheral nervous plexuses are the seat of the painful sensation, sometimes the perforating twigs of the cutaneous nerves, and sometimes the larger branches and even the trunks of the nerves. There may be a gradual transition from a feeble itching to a pruritus which vibrates through the whole nervous system, or to a state of neuralgic pain such as has suggested the term eczema neurosum.

When the zinc-ointment, with the addition of spirits of wine, camphor, tannic acid or carbolic acid, fails to relieve the pruritus, we may sometimes meet with help from hot water, from a cold starch-poultice, from water-dressing; from lotions of soda, hydrocyanic acid, juniper tar, or a solution of nitrate of silver. But all these remedies must be regarded simply in the light of

adjuvants to the principle of treatment already discussed; and, when the neuralgia is decided, we must seek for relief in constitutional means.

The stavesacre ointment is often very successful in the relief of the pruritus which sometimes follows scabies; and as this remedy, next to sulphur ointment, is one of our best means of destruction of the acarus, it may possibly happen that the pruritus is protracted by some such overlooked and unlooked-for complication. The pruritus of lichen urticatus is best relieved by Hebra's lotion—a lotion consisting of an ounce each of soft soap, juniper tar, and alcohol, diluted with five ounces of water. And the sometimes ungovernable itching of lichen planus is with the most certainty mitigated with hydrocyanic acid suspended in an emulsion of bitter almonds.

Another modification of the local treatment of eczema is the natural consequence of its seat. Neither powder, nor lime-water lotion, nor zinc-ointment, would be suitable for application to the eyelids, or within the meatus aurium; but for both these regions an excellent substitute will be found in an extremely diluted nitrate of mercury ointment, or in the glycerine of tannic acid of the *British Pharmacopœia*. Again, the oxide of zinc-ointment is unsuitable on the scalp, in consequence of clogging the hair, and thereby giving rise to much inconvenience; but, in the latter situation, a far better remedy presents itself—namely, the nitric oxide of mercury ointment, diluted with benzoated lard in the proportion of one part to three of the diluent. The case of dermatitis exfoliativa (No. 6), suggestive of an extensive burn, was most efficiently relieved by the linimentum calcis and the carron-oil.—*British Med. Journal, March* 25, 1871, *p.* 303.

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## 73.—ON ECZEMA IN CHILDREN.

By Erasmus Wilson, Esq., F.R.S., Professor of Dermatology in the Royal College of Surgeons.

It is important to remember that eczema prevails at every period of life, from infancy to extreme old age, and, consequently, that the treatment will be very much influenced by the age and constitution of the patient. As a general expression, it may be stated that the eczematous affections take their origin in lowered vitality or debility, and that the aim of our treatment should be—to restore health. But the conditions of debility necessarily vary at different periods of existence. In infancy and childhood, the cause of debility will be defective nutrition or defective nutritive power; in the adult and at middle age, the cause will very probably be malassimilation from derangement of the digestive functions; while in old age and in certain

adults of highly nervous temperament, the cause of eczema will be a malassimilation determined by irritability and waste.

Thus, with a patient before us suffering under eczema infantile, we must inquire carefully into the diet of the child: it has probably failed to obtain its natural food, and recourse has been had to bringing it up by hand. Then we must draw on our resources bearing upon the nutrition of infancy—the substitutes for mother's milk, the wheaten flour, the beef-tea, the cod-liver oil; in such a case the best nurse will prove to be the best physician. A similar principle of treatment carries us through the whole of the nutritive period of life from infancy until the full development of the body is perfected by growth. We may find many intercurrent sources of debility intruding upon the steadfast stream of our course; but our aim should always be to perfect, as far as we are able, by diet, by hygienic measures, and by medicines, the healthy nutrition and development of the organisation. We may be interrupted by the successive trials of vaccination, of teething, of infantile diseases, of scanty or deficient food, of growth; but we must be ready to appreciate every condition predisposing to exhaustion, and apply in good season and judiciously the appropriate remedy.

If the powers of constitution of our patient be equal to the proper digestion and proper assimilation of the improved diet, our purpose is accomplished, the cure is certain and rapid; but the nutritive power may be weak, in which case it will be necessary to find some means of giving it strength. Such a means we possess in arsenic combined with iron; and there is no medicine more harmless, more certain in its effect, and more successful, than arsenic. The dose for an infant of a month or six weeks old may be one minim of Fowler's solution, equal to the $\frac{1}{120}$ of a grain of arsenious acid—a dose far too minute to do harm, although capable of doing wonders in the way of good; and we may in the course of a few days increase the dose to a minim and a half or two minims. I have for many years prescribed arsenic in a combination, which I have found so safe, so efficacious, and so convenient, that I have been unwilling either to vary the remedy or the formula. The latter is as follows.

℞. Vini ferri ℥iss; syrupi simplicis ʒ iij; liquoris arsenicalis ʒ j; aquæ anethi ℥ij. Misce. One drachm, with meals, three times a day.

Let me recapitulate: firstly, we should cover every visible part of the eruption, whatever its state, and avoiding only the hairy scalp, with the benzoated zinc-ointment in combination with spirits of wine; secondly, we should examine carefully into the diet, and direct such a regimen as in our opinion is most likely to be nutritive; and thirdly, we should administer

from one to two minims of Fowler's solution in the combination already mentioned, three times in the day, with the single condition that the medicine should be given on a full stomach, and stopped immediately if it chance to disagree. Two other points, but of secondary importance, follow upon this course of treatment. It is well to see that the bowels act regularly. With an unsuitable diet it is hardly probable that digestion will be complete; but nothing short of constipation need delay the commencement of the curative treatment.

The other point opens up a question as to the derivative influence of eczema: that it is derivative in infancy is more than improbable, and I should no more hesitate to arrest an ichorous discharge from the skin than I should a chronic diarrhœa from the intestinal canal. Nevertheless, it is always wise to be on our guard; and if any apparent inconvenience arise from the sudden closure of the outlet through the skin, it might immediately be removed by the administration of one grain of calomel with one grain of sugar, dropped into the mouth or upon the tongue.

The usual issue of the treatment now mentioned is a speedy and effectual cure; and very rarely, indeed, have I met with a chronic prolongation of the disease, and even more rarely with a fatal termination of the case. Whenever the latter has occurred, it has resulted from bronchitis or convulsions. Bronchitis is intimately associated with eczema—in fact, is neither more nor less than an eczema of the bronchial mucous membrane, and of course has its dangers; whereas convulsions may seize on the delicate organisation of infancy, excited by a very trivial cause, such as mere indigestion or the cutting of a tooth, and cannot therefore be regarded as a direct consequence of eczema. On the contrary, it may be reasonably predicted that convulsions would be less likely to attack an eczematous child than one in apparent good health.

In eczema, as it occurs at the age of childhood, and thence upwards to early manhood, the principle of constitutional management already described, namely, the restoration of power by diet, by hygienic measures, and by tonic remedies, may be regarded as the universal plan of treatment and unfailing in its results.—*British Med. Journal, April* 1, 1871, *p.* 332.

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## 74.—ON THE USE OF THE ECRASEUR FOR THE REMOVAL OF NÆVOID GROWTHS.

By JAMES F. WEST, Esq., Senior-Surgeon to the Queen's Hospital, and Professor of Anatomy in Queen's College, Birmingham.

[Mr. West has been in the habit of using the ligature, chloride of zinc paste, or injection of perchloride of iron for many years,

until he met with a case in which the ligature failing entirely he was induced to try the écraseur.]

The destruction of nævi by caustics is attended by uncertain results, and the consequent cicatrices are often deep and ugly, from the impossibility of our gauging the distance to which the caustics—as chloride of zinc, nitric acid, &c.—ought to penetrate the tissues.

The ablation of erectile tumours is probably the most perfectly reliable means of treatment, and this may be accomplished either by enucleation, the ligature, the knife, or the écraseur.

Piecemeal excision or enucleation is often attended with great loss of blood, even where the adjacent arterial trunks have been compressed as completely as possible; and the little patients who are the ordinary subjects of nævi bear hemorrhage badly. A comparatively trifling loss often proves so serious to the patient that the attacking of large subcutaneous nævi by this process would hardly be justifiable.

The same difficulty meets us in the use of the knife; and I cannot doubt that the older surgeons were just in laying it down as a rule that, in removing nævi, it was always proper to cut wide of the tumour, and on no account to cut into its mass.

The introduction of either hare-lip pins or of ligatures frequently fails to cure; the latter are especially unreliable with venous nævi of large size, owing to their becoming loose, even though the skin around the growths may not have been included in them. The parts daily diminish in size, so that ligatures have to be again and again applied to ensure the entire destruction of the tumour. Moreover, ligatures often set up troublesome ulceration at the base of the nævi, from which occasionally severe hemorrhage takes place.

The advantages which, in my experience, the écraseur offers are, that hemorrhage is avoided—an important element in all operations, but particularly so with children, and that you have a linear cicatrix and a comparatively small wound; and thereby prevent or diminish the deformity which, by other operative procedures, will almost of necessity be produced. Chassaignac, also, claims for it that less inflammatory action and less suppuration attend its use than that of the knife; and, consequently, that the wounds resulting therefrom heal more readily, and are less likely to be followed by pyæmia. On these latter points I will not now offer an opinion; but as to the smallness of the resulting cicatrix—a great desideratum in all operations about the face,—and as to the freedom from hemorrhage, even when dealing with large growths of this kind, I am quite decided.

*Case* 1.—Mrs. L.'s child, aged seven months, had a purple subcutaneous nævus, as broad as a crown-piece, on the left side of the head, which, when filled with blood, was elevated to the

size of a large walnut. Pressure with elastic pad had been tried, but without success, when Dr. Suffield, the ordinary medical attendant, asked me to see the case. I ligatured the tumour in four segments on Nov. 11th, 1868; but, owing to the ligatures becoming loose, very little reduction in the size of the tumour took place. On the 18th I encircled the base of the nævus with a ligature. On the 22nd I introduced two strong pieces of whipcord through the centre of the tumour, and tied it in two segments. These also became slack; and very little benefit having been obtained, I resolved on using the écraseur. Having first passed two hare-lip pins through the base of the tumour, I applied the chain of the écraseur below them, and, gradually working the handle of the instrument every twenty seconds, succeeded in removing the whole mass, which was as large as a walnut, in the course of fifteen minutes. No bleeding of consequence occurred, the wound healed up rapidly, and a very small cicatrix, which was perfectly free from all trace of nævoid tissue, resulted.

*Case* 2.—Elizabeth L., aged forty-seven, was admitted into the Queen's Hospital on April 26th, 1869, with a dark pigmentary nævus on the right cheek which had existed from birth, but had recently increased greatly in size, and which reached from the outer canthus to the angle of the jaw, and measured two inches and a half in length by one in breadth. I removed it with the écraseur the same day, first making an incision through the skin around the tumour to the depth of a quarter of an inch, in which the chain of the instrument might lie. This procedure facilitates the operation, as the écraseur, though it freely divides mucous membrane, rather tends to tear the integument, and is at any rate some time in cutting through it. Three silver sutures were introduced after the operation. These were taken out on May the 1st, and two days after, the wound being perfectly healed, she went home, having a very small linear scar on her cheek.

*Case* 3.—Mrs B., of Bromsgrove, came to me with a strawberry-like pigmentary nævus as large as a chesnut on the left cheek, just below the zygoma. Dr. Mackey, Professor of Materia Medica in Queen's College, gave chloroform, and I applied the écraseur, first making a slight incision in the skin around the tumour as in the former case. Scarcely any blood was lost by either patient. Three silver sutures were introduced, which were taken out on the fourth day, and on the seventh day after the operation she was able to return home. This growth also had existed from birth as a mole, but had lately developed greatly and become very vascular and sensitive. The cicatrix which resulted was linear, and was so slight

that when she presented herself in my consulting room a few months after, I did not at first distinguish it at all.—*Lancet, March* 4, 1871, *p.* 303.

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## 75.—ON THE TREATMENT OF CARBUNCLE.

By Dr. J. MURRAY, M.A., Wickham, Hants.

[The usual treatment of carbuncle by free incisions, although in some cases productive of relief, is not in the majority of cases satisfactory. Many surgeons prefer to abstain from all operative interference.]

About five years ago I was accidentally led to adopt a plan of treatment new, so far as I am aware, and free from all the objections to which incision is open, but which expedites in a very remarkable degree the process of recovery.

*Case* 1.—12th June, 1865. I was consulted for the first time by an unmarried lady, 36 years of age, of good constitution, but nervous temperament, and much exhausted by pain and sleeplessness, due to fissured anus, symptoms of which had appeared three months previously. For three days severe pain had also been felt within the vulva, and on the mucous surface of left labium a carbuncle about an inch and a quarter in diameter was found. From its whole area the mucous layer had peeled off, leaving a smooth shining surface of a livid colour, surrounded by the ordinary halo of reactionary inflammation. With the certain prospect of one cutting operation, incision of the carbuncle was so strongly objected to that I determined instead to imitate the ordinary procedure by dividing the tissues with potassa fusa. Accordingly, with a sharp-pointed piece I crossed the surface very lightly, intending to complete the process next day, when the amount of destruction produced by the first application had been ascertained In order to prevent all further escharotic action, the parts were freely sponged with dilute vinegar, and a fold of oiled lint was then introduced between the labia.

13th. The vulvar pain rapidly diminished after the application of the caustic, and is now quite gone. All swelling has disappeared, and the site of the carbuncle is only marked by a bright-coloured patch of surface three quarters of an inch in diameter, crossed by two shallow linear depressions.

15th. There has been no further discomfort due to the carbuncle, and no perceptible trace of it remains. Sphincter ani incised.

*Case* 2.—5th August, 1865. J. P., labourer, aged 33, flabby and ill fed, has had, for ten days, a painful swelling at the back of his neck, and during the last week this has rendered him

unable to work. Extending downwards from the roots of the hair in the middle line, there is now a carbuncle four inches in diameter, boggy, and surrounded by little inflammation. Its surface is studded with openings of various sizes, occupied by grey sloughs, and exuding their sanious pus. This is now scored crucially with potassa fusa, a superficial eschar in each direction being produced. Linseed poultice ordered.

6th. The area of swelling has diminished by nearly one-half, most of the sloughs have separated, and the corresponding openings have assumed a healthy aspect.

7th. No sloughs now remain, the general surface looks comparatively healthy, and the openings are rapidly healing. To apply water-dressing. Resumes work.

12th. There are still a little redness and induration, but no traces of caustic action are perceptible.

Up to the present time I have treated all my cases of carbuncle in the same way, and with such uniformly similar results that it would be unnecessarily tedious to give details of each. I shall therefore pass over the next ten of the series, and confine myself to describing the last three, which are representative of the disease in its various stages, and illustrate well the usual progress under treatment. Case 13 was a favourable one for observation, and I delayed interfering until the patient's welfare became endangered.

*Case* 13.—20th July, 1870. S. M., aged 21, has for two years had disease of the right hip-joint. Partial anchylòsis has taken place, but large chronic abscesses have formed over lumbar and sacral regions. Until quite recently, however, he has been able to take exercise upon crutches, with the additional support of a gutta percha splint. Three days ago he found he could not apply the splint in consequence of the appearance of a painful pimple over trochanter major. A carbuncular swelling, half-an-inch in diameter, is found to have developed itself, and this he is ordered to dress with benzoated lard, merely in order to prevent irritation by friction of clothing.

25th. The area of furuncular inflammation has steadily increased, and now there is over the trochanter and its immediate neighbourhood, a tense circular swelling nine inches in diameter, the centre of which is occupied by a carbuncle four inches across. The latter is apparently rather more than an inch in thickness, and the epidermis has peeled off, leaving a livid shining surface perforated by numerous small holes, from which clear serum exudes, and in each of which can be seen the white tip of a core. Surrounding this is a livid and brawny halo. No sleep has been obtained for two nights, and patient is becoming exhausted. Potassa fusa applied in the usual manner.

26th. 10 a.m. Pain rapidly diminished after caustic was employed, and a good night's rest was had without taking an optional dose of chloral hydrate. During the night a serous discharge, sufficiently profuse to saturate several folds of sheeting and penetrate to the bed beneath, proceeded from the carbuncle. On examination the potash scores are found to be the source of this. The surface of carbuncle has assumed a healthy red colour, and its diameter is reduced to two inches. Two narrow grooves at right angles to each other, with pale granulating surfaces and pink healthy margins, represent the results of caustic action.

28th. No trace of carbuncle, except a slight superficial blush remains. Patient is able to re-apply splint and use his crutches.

3d Aug. For two or three days some discomfort has been felt, and now there is over trochanter a superficial ulcer about an inch in diameter, and of healthy appearance, probably due to premature application of splint.

8th. Ulcer has quite healed.

*Case* 14.—15th Aug., 1871. A. W. Page, aged 17, has a furuncular swelling on chin, an inch in length and half an inch in breadth. It first appeared four or five days ago, and has since been gradually increasing in size. Potassa fusa applied.

20th. The pain and swelling rapidly diminished, and now there is no trace either of tumour or eschars.

*Case* 15.—12th Oct., 1870. J. W., aged 55, labourer. During the last ten days has had a painful swelling over patellar tendon, which has rapidly increased and now prevents his walking. This consists of a carbuncle in the boggy stage, three inches in diameter, with a large slough in the centre, and small suppurating openings over its surface. Considerable reactionary inflammation surrounds it, and both leg and foot are œdematous. Potassa fusa applied.

22d. Œdema and inflammation have subsided and slough has separated. In the site of the latter locality granulation is taking place. Intends to resume work to-morrow.

At an early period I mentioned the matter to my friend Mr. Pater, Hants County Lunatic Asylum, who gives me the results of his experience as follows:—

"During the last four years all the cases of carbuncle occurring in this asylum—about 20 in number—have been treated by forming superficial eschars by means of potassa fusa, and with uniformly satisfactory results. In cases where sloughing has not already set in, resolution takes place without suppuration; and in more advanced cases healthy suppuration is rapidly established, and speedy separation of sloughs takes place."

Incision in carbuncle is generally, I think, advocated upon grounds which imply a mistake as to the pathological condi-

tions present. The professed object is to relieve tension and give exit to sloughs. Now, the tension is chiefly due to the cores or plugs contained in the meshes of the corium, and therefore interstitial, so that only the tissues immediately divided can be relieved by incision. Again, no sloughing occurs until, as in other cases, the inflammatory action has been so severe as to produce mortification. The core has nothing in common with sloughing cellular tissue as is taught in some surgical text-books. It is simply a fibrinous coagulum, the product of inflammation going on in the surrounding cellular tissue. In what manner these conditions are altered by the caustic action it is impossible precisely to say; but in some way or other the nutrition is so modified that the inflammation subsides and the cores are re-absorbed. Dr. H. Blanc has observed analogous effects in diffuse cellulitis.—*Glas. Med. Journal, Nov.* 1870, *p.* 66.

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## 76.—ON THE TREATMENT OF CARBUNCLES.

By Dr. WILLIAM MARCET, London.

The first sign of a carbuncle is usually denoted by a sensation similar to that produced by a slight prick with a pin, occurring when the hand is passed over the affected part. Nothing may be seen yet on the skin, but some few hours later a small vesicle appears, when the pain becomes more acute, and may be caused by the mere contact of the clothes. If the carbuncle be allowed to proceed, say, for twelve hours beyond its very first appearance, it will run its usual course; but its progress may be arrested by the early destruction of the vesicle and its contents by means of the cauterising action of heat. I have adopted many plans to effect this purpose; but the simplest of all, and one which may be considered as always at hand, is the use of an incandescent lucifer-match. The vesicle is to be merely touched, for a fraction of a second, with the red-hot point from five to seven or eight times in succession, when it assumes a dull-whitish appearance from the coagulation of the albumen it contains. The end of a hot wire may also be used. The pain of the operation is really trifling, and it will save from a week to a fortnight's suffering. I have repeatedly applied this form of actual cautery to myself, and shall not hesitate to do so again if necessary.

In general, within four or five hours after the operation, the pain from the incipient carbuncle has in a great measure disappeared, and there is an end to it. It may happen, however, that the carbuncle, at its origin, is deep under the surface of the skin, when no vesicle appears. I have not been so successful with the use of the actual cautery in these cases as in the

others; but probably, had the cauterisation been carried deeper, the mischief might have been arrested.

The present treatment suggested itself to me from a consideration of the nature of the vesicle which precedes a carbuncle. This vesicle appeared to me to contain a virus, which was the real cause of the subsequent inflammation, and I concluded that by destroying this virus the carbuncle might be "nipped in the bud." I have tried the local application of nitrate of silver and nitric acid, but they cannot be relied upon, and I fully believe that nothing will act so satisfactorily as the cauterising action of heat.

I may observe that, for the treatment to be successful, it should be applied as soon as possible after the first appearance of the carbuncle. The progress of boils in this country could probably be arrested in the same way, and it might be worth the while to try whether Indian boils might be cured by the present means.

[Our plan is to take up a fold of skin and nick the vesicle, taking care to cut well through the base, and when deep under the skin turning out with the back of the bistoury the small pellet of matter which there exists. The spot should then be covered with a morsel of lint soaked in blood, and over that a piece of plaster to retain it in position.—EDS. RETROSPECT.] —*Lancet, Jan.* 14, 1871, *p.* 45.

## SYPHILITIC AFFECTIONS.

### 77.—GONORRHŒA TREATED BY TANNIN AND GLYCERINE.

Dr. Schuster of Aix-la-Chapelle describes, in Pick and Auspitz's *Archiv f. Dermatologie und Syphilis*, a method of treating gonorrhœa and gleet, to which he was led by observing the results of a similar plan in uterine catarrh. Tannin, when mixed with glycerine, forms a waxy mass, which soon becomes smooth, hard, and brown, but readily dissolves under a gentle heat. Dr. Schuster has made rods, three or four inches long, consisting of tannic acid 2 parts, powdered opium 0.12 part, with a sufficiency of glycerine. These are soft in the summer, but become very brittle in the winter. The rod, moistened with hot water, is introduced into the urethra, and a piece about an inch and a half long is left in: it melts down and forms a whitish mass with the mucous secretion of the canal. The rod may, after remaining from five to ten minutes, either be removed by the finger or be expelled by the stream of urine. The remedy is applied twice or thrice daily. Dr. Schuster treats gonorrhœa in all its stages in this way; a cure follows in a period varying from seven to eighteen days.—*British Medical Journal, Jan.* 14, 1871, *p.* 39.

## 78.—TREATMENT OF GONORRHŒA BY INJECTIONS OF PERMANGANATE OF POTASH.

By Dr. W. MACFIE CAMPBELL, House Surgeon, Seamen's Hospital, *Dreadnought*.

[The writer, in the following short article, contrasts the results of treatment by injections of permanganate of potash with those of other modes of treatment employed during the two preceding months.]

In sixteen cases treated in this way, there have been ten cures at periods of from two to ten days, and six cases discharged as convalescents, they feeling well enough to leave hospital, but not being such as I could class as cures. Thirty-one cases in all were treated in hospital, with the results which I can most readily show in the following table.

| No. of cases treated. | Treatment employed. | Average length of stay in hospital. | Cases cured. | Cases convalescent. | Average time of cure. | Average duration of disease before treatment. |
|---|---|---|---|---|---|---|
| 6 | Cubebs and copaiba | 12 days. | 4 | 2 | 13 days. | 5 days. |
| 2 | Condy's injections, two drachms to the pint | 9 ,, | 1 | 1 | 8 ,, | 9 ,, |
| 3 | Tincture of muriate of iron internally | 8 ,, | 1 | 2 | 10 ,, | 214 ,, |
| 3 | Sandal wood | 13 ,, | 1 | 2 | 8 ,, | 74 ,, |
| 1 | Soluble bougie of gallic acid | 5 ,, | ... | 1 | ... | 150 ,, |
| 16 | Permang. of potash injections, 5 grs. to the ounce | 6 ,, | 10 | 6 | 5 ,, | 45 ,, |

These figures, I think, compare very favourably. It may be observed that the injection of permanganate of potash is useful both in acute and chronic cases, but yields the palm to the iron treatment for very old gleets; while copaiba and cubebs are useful in acute, but quite useless in chronic cases, and even in acute cases taking more than twice the time for cure. The injections of Condy's solution and water, as generally employed, are too weak to do much good. Soluble bougies of gallic acid I have only used in one case, which, of course, cannot be argued from; but are worthy of more trial. The injection I employ generally is of five grains of the permanganate to the ounce of water; six to ten grains I have also used, but with no additional advantage. Only two patients have complained of pain during the use of the injection, and that was easily stopped by omitting its use for one day. In the ten-grain cases there was no complaint at all. I may add that the patient is better to be kept in

bed, and should continue the injection for at least a week after apparent cure.

These results are, I submit, quite sufficient to induce a trial of this treatment. The patient's stomach is not put out of order by nauseous drugs, and few patients would grudge the time necessary (four times a day) for so pleasant a cure.—*Lancet, Jan.* 14, 1871, *p.* 73.

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## 79.—PERMANGANATE OF POTASH IN THE TREATMENT OF GONORRHŒA AND GLEET.

By Dr. THOMAS WARDEN, Royal Naval Hospital at Haulbowline.

[Dr. Warden's belief is that gonorrhœa and gleet may be cured in a couple of days by the permanganate of potash treatment, although from the rarity of gonorrhœa in the Mediterranean he is unable to adduce any considerable number of cases to prove it.]

On the sick-list of the *Hibernia* I found a marine who had been under treatment for twenty days, which consisted in pursuing the recognised plan of giving salines, copaiba, and injections of different strengths and substances, with the usual results in these tedious cases, where the constitution is beginning to suffer from the long-continued discharge and confinement. This case was really becoming an opprobrium, and we felt anything but satisfied with our treatment. I decided then on trying the permanganate of potash, which I tentatively gave in five grains to the ounce as an injection at least four times a day. This gave rise to no pain or other inconvenience. Since then I have administered it in as many as fifteen grains to the ounce. The result in this case was that the man was well in two days, or after about eight injections.

In the course of a fortnight this man went on leave, and came back with gonorrhœa. The same treatment was pursued, with a precisely similar result. I prescribed it also in the case of an officer who had had the disease for six weeks. The discharge in this case was more gleety, and his health was beginning to suffer. On this occasion the cure took six days, but I do not think he attended fully to the directions I gave him; neither was he regular in its use.

I regret that this is all the evidence I can bring of the good effects of the permanganate; but I see it is needless for me to wait for more cases, so I place this imperfect experiment before the profession. I do not vaunt these cases as being conclusive, because their paucity will hardly warrant me in claiming for this remedy the name of specific; but I bring them forward in order that those who have more opportunities of treating the

disease may be tempted to try the permanganate; and I hope the results will be as satisfactory in their practice as they have been in my three cases. From what I have seen of its action, I feel very sanguine concerning its efficiency. I only ask that those who make use of it will be kind enough to publish their cases, whether attended by success or the reverse.

Now a few words may be necessary as to the method of using the injection. It must be remembered that the permanganate of potash loses its virtue by admixture with any extraneous matter, or exposure; therefore it is advisable to mix the injection immediately before using it. This ought to be done in either a glass or porcelain dish, perfectly free from any greasy matter; and the syringe must be in a like state of purity. If care be not taken in its use the patient's linen will be stained; and this should be remembered in private practice.—*Lancet*, *Dec.* 3, 1870, *p.* 779.

# MIDWIFERY,

## AND THE DISEASES OF WOMEN AND CHILDREN.

### 80.—PRACTICE IN THE PREDICTION OF THE DAY OF CONFINEMENT.

By Dr. J. MATTHEWS DUNCAN, Edinburgh.

[The end of last menstruation is generally taken as the point to count from, in calculating the duration of pregnancy; but this mode of calculation is purely empirical.]

In my former papers, which form part of my work on Fecundity, I use the great recorded experiences of Merriman and Reid. Estimating by these, I find that the 278th day after the end of last menses is the average day of delivery at the full time; and on this I proceed. No ingenuity can devise a superior plan of estimating, so long as the last menstruation forms the only generally available *terminus a quo*. The introduction by many authors of scientific views into the question of the best way of predicting the day of confinement, may be justly characterized as either at least unnecessary or else merely pedantic. Till I find a larger and more carefully compiled mass of facts than those of Reid and Merriman, I shall adhere to my method of calculating based on the circumstance that 278 days is the average interval between menstruation and parturition; and in doing so I have science and common sense on my side.

The method which I recommend is confessedly a rough one. The calculation itself is always what is called a rough one. My method certainly is loose and erroneous to the extent of one day in certain cases, which I have specified at page 340 of the first edition of my work on Fecundity already referred to. The plan is simply as follows:—Find the day on which the female ceased to menstruate, or the first day of being what she calls "well." Take that day nine months forwards as 275 days, unless February is included, in which case it is taken as 273 days. To this add 3 days in the former case, or 5 if February is in the count, to make up the 278. This operation is perfectly simple, and so easy of performance as to render a periodoscope quite useless.

Now, any practitioner can test this plan by his own experience, in a purely synthetical and reverse manner. He can try the plan, and then see how it has led him; whether it has led him and his patients into error or not. Since I adopted this reverse method of verifying my plan of calculation, I have found that Ahlfeld had already resorted to a similar test. It is only very slightly different from the method by which the plan of calculating was developed. The difference is stated as follows:—Cases of delivery collated yield results on which the plan is founded: instances of prediction compared with the real events test the plan.

I shall now show what my predictions on this plan have come to. No one can hope to be an absolutely good prophet in this matter, but we can be as good as possible, as nearly right as may be. The predictions to which I shall immediately make reference were all written down before the events, and remain written. I have only 153 cases to refer to, all collected within several recent years. They are few, because I did not venture on the written-down prediction unless I was satisfied that I got good information as to the day of the cessation of the menses.

I need scarcely repeat, that in practice I do not predict a day, but a week. I predicted a day in my note-book for my own use. These 153 predictions in my note-book I now analyze.

In 10 cases the day of confinement was exactly predicted, or about once in every 15 cases.

In 80 cases the confinement took place sooner than was predicted. The number of days of anticipation was, for the whole 80 cases, 590, or an average of above 7 days for each case.

In 63 cases the confinement took place later than was predicted. The number of days of protraction was, for the whole 63 cases, 535, or an average of above 8 days for each case.

In 63 cases, or more than one-third of all, the time of confinement was successfully though not exactly predicted, the birth occurring not earlier or later than 4 days from the predicted day.

The average error was about 7½ days,—a circumstance which indicates that the prediction should not state the week of confinement but the fortnight of confinement, there being generally an error of a little above 7 days on the one side or the other of the ascertained average day.

But the most interesting result of these figures is the answer to the question, Can the calculation be improved? and the answer is, that it is, for practical purposes, perfect, or as nearly so as the present state of science permits. This near approach to perfection is shown, firstly, by the observation, that the errors on either side of the predicted day are nearly equal. If the errors on either side were exactly equal, then the calculation

would be perfect; for it would thus be shown that, for the mass of cases, the exactly most probable day of confinement had been hit upon. In my 153 cases the excess of error is on the side of anticipation. This excess is 55 days. Now, 55 days for 80 cases is less than a day of average error; and as our prediction does not pretend to even the accuracy of a day, the error may be truly regarded as trivial.—*Edinburgh Medical Journal, March* 1871, *p.* 788.

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## 81.—A CASE OF INDUCTION OF PREMATURE LABOUR BY MEANS OF THE UTERINE DOUCHE.

By W. WHALLEY, Esq., Bradford.

[The patient had had thirteen previous pregnancies. The labours had always been lingering and difficult. On the last three occasions craniotomy had been resorted to. There was a diminution of the cavity of the brim of the pelvis in the conjugate diameter.]

The patient consulted me again in August last, when about six months and a half advanced in pregnancy. With some difficulty I obtained her consent to allow premature labour to be induced at the seventh month. Accordingly, on September 7th, at 1 P.M., being exactly seven months since she last menstruated, I injected, by means of Barnes's syringe, a gentle stream of warm water against the os uteri for about five minutes. On September 8th, at 5.30 P.M., I introduced the tube of the syringe about an inch within the os uteri, external to the membranes, carefully detaching the latter, and then injected a stream of warm water for about six minutes. On the following morning, about 6.30, I was summoned to the patient, and found the os completely dilated and the membranes protruding into the vagina. On rupturing them a foot and funis presented; the pains were strong and frequent, yet more than forty minutes elapsed before delivry could be effected. The fœtus was well formed, but exhibited no signs of life when born. The usual means of resuscitation were practised for a considerable time, but proved unavailing. The mother made an excellent recovery.

The above case affords additional evidence of the simple, easy, and efficient mode of inducing premature labour by the aid of the uterine douche. Had the presentation been natural, the child would undoubtedly have been born living. Notwithstanding this disappointment, it was gratifying to find that the patient, who had been three times subjected to all the suffering and danger attending the operation of craniotomy, fully appreciated the success of the operation.—*Lancet, Nov.* 26, 1870, *p.* 740.

## 82.—ON THE ADVANTAGES OF THE EARLY USE OF THE LONG FORCEPS.

By Dr. F. H. DALY, Dalston, London.

[The writer first states his belief that many lives are lost by procrastinating the use of the long forceps in cases of contraction at the brim of the pelvis. In illustration he brings forward two cases, the second of which we give.]

The patient, a primipara, æt. 22, sent for her medical attendant on the morning of the 4th of June, 1868, at eight o'clock A.M. She had been having slight pains all night; he found the pains becoming stronger, and the os about the size of a crown piece, the membranes not ruptured. The woman was ill-shapen, and the pelvis peculiarly small, but having no appreciable deformity beyond the general smallness. Mr. Ryder visited the patient several times during the day; the os dilated somewhat, but got fixed between the brim of the pelvis and the child's head. He being satisfied that the head could not pass the brim, asked me to see the case with him. I found the pains very severe and long, and the woman getting very impatient; the os was, as stated above, fixed between the head and brim. So small was the pelvis, that we both feared the undiminished head could never pass; however, we determined to wait some time longer before interfering. At 12.30 A.M. we again met, the case had altered in no particular, except that the patient was getting more and more restless. We therefore determined to give chloroform, and try the long forceps without further delay; but the patient, although most anxious to be delivered with instruments, would not have chloroform. I therefore proceeded to apply the long forceps without the anæsthetic; but in consequence of the extraordinary smallness of the pelvis, and the high position of the head, it was only after several failures that I succeeded in getting on the second blade and locking the instrument; then having tied the handles together I commenced making traction. For more than half an hour Mr. Ryder and myself, relieving each other, used traction with quite as much force as was justifiable, without making any very perceptible result, and it was nearly an hour and a half from the time I got the forceps locked, until I delivered her of a male child about the average size, having its head considerably and curiously elongated from the pressure and traction.

Mr. Ryder informs me that, upon his calling on the third day afterwards, his patient was sitting up in bed having a chop, and the child's head was the natural shape. She was up by the end of the week, and never had a bad symptom.

*Remarks.*—My apology for bringing these two cases before this Society is the fact that general practitioners, as a rule,

delay much too long before applying the forceps, and especially the long instrument, and my belief that an expression of opinion by some of the leading Fellows of the Obstetrical Society would naturally have much weight, and induce a more early use of the forceps, when the head is *above* the brim of the pelvis, than is generally recommended in the text books, and, as a result, the saving of many valuable lives. I know, as a practical fact, that a large section of medical men only resort to the long forceps when the patient is *in extremis*, and as a consequence, if the patient survives an operation undertaken when she is in a state of exhaustion, she runs the risks consequent upon allowing the uterus to wear out its contractile power, such as hemorrhage, or the absorption of purulent or other poisonous matters.—*Obstetrical Transactions*, 1871, *p.* 4.

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## 83.—ON THE MECHANISM OF THE EXPULSION OF PLACENTA.

By Dr. J. MATTHEWS DUNCAN, Edinburgh.

Every one knows that the membranes are expelled inverted or flapped over upon themselves; and the same view is held regarding the placenta. It is to this last part's condition during expulsion that I am now directing attention.

The wall-plate of Schlutze is an admirable representation of the expulsion of the placenta as it occurs in the first two modes described by Baudelocque; and I may add that this author's descriptions have been repeatedly copied. Baudelocque, drawing chiefly on his imagination for his supposed facts, says:—"Sometimes this separation begins at the centre of the placenta, and sometimes at some point of the circumference, which produces different phenomena. In the former case, the middle of the placenta being pushed forward, it forms a bag behind which fills with blood, and it presents that side to the touch which is covered with the membranes and vessels. The placenta forms nearly a similar bag, and presents in the same manner, when it begins to separate from the uterus at that part of its edge which is furthest from the orifice. But things go on very differently when the separation begins at its lower part, especially if it be in the neighbourhood of the orifice. In this latter case the placenta rolls itself up in the form of a cylinder, and according to the length of the uterus, so as to present its anfractuous surface to the touch; and its exit is always preceded by a little fluid blood." Schultze goes a little farther than Baudelocque, and actually describes the accumulated hemorrhage from the uterine sinuses as co-operating to push down the already almost completely detached placenta and complete its separation,—a view so

utterly unsupported by observation or argument, and so unlikely, that I shall not say anything more regarding it.

Now, the erroneous belief that the placenta generally descends presenting its fœtal surface seems to me to have arisen from observers not keeping in mind the very great frequency with which the natural mechanism of delivery of this cake is interfered with. I may say that it is unfortunately the rule to interfere with this part of the natural mechanism of delivery. Such interference, generally carried out as it is by pulling the cord, produces an unnatural mechanism—inversion of the placenta, as Ramsbotham calls it and this unnatural mechanism, this inversion of the placenta, comes to be described as the natural conduct of the delivery. The placenta, says Rigby, "descends into the vagina inverted, *i.e.*, with its fœtal or amniotic surface turned outwards. Whether or not," he adds, "this is produced by pulling on the cord, is perhaps a question."

To find out the natural mechanism of the expulsion of this cake, it is only necessary to watch the process as nature conducts it; that is, in cases in which the practitioner does not try to modify it in any way. This any one can easily do, by wounding or otherwise marking the part presenting at the mouth of the womb, and then after its birth examining the placenta to find where the wound is; or the observer may pinch the part first presenting at the vaginal orifice, and retain hold of it till the whole is born, and then find what is the part so pinched.

In this way it is easily discovered that the part of the placenta presenting at the os uteri, and subsequently at the os vaginæ, is not the fœtal or amniotic surface, but the edge of the placenta, or a point very near the edge. When it is not exactly the edge, the placenta is not inverted or folded upon itself, there is only a little of the lower marginal part of the cake transversely folded up; it is still really the edge that presents, only thickened a little by being folded on itself; and I think this folding occurs chiefly in placentæ which are thin at the part folded. This folding is manifestly caused by the pulling up of the edge by the still adhering membranes; the resistance of the force required for their separation being greater than the rigidity of the marginal part of the placenta so folded.

My own numerous observations satisfy me that the inversion of the placenta, or its folding upon itself transversely to the passage, or the presentation of its fœtal surface, as authors describe, and as Schlutze and others depict, is a very rare occurrence,—so rare as to debar describers from calling it *a* natural, and still more from calling it *the* natural, mechanism. The placenta is folded upon itself during the process; but the folds are according to the length of the passage, not transverse to it, as inversion or presentation of the fœtal surface imply.

These remarks are remarkably corroborated by the memoir of Dr. Lemser on the Physiological Separation of the Placenta. This gentleman's observations appear to me to be inconsistent with the views of Baudelocque and Schultze as to the separation of the placenta, and they are, undoubtedly, the best observations on the point which we possess. But it is more interesting, with a view to the present discussion, to remark that Lemser always describes the border or edge of the placenta appearing in the os uteri as the ordinary result of separation and propulsion of the cake.

The advantages of the natural mechanism, as I have described it, are obvious. It is true, that after the passage of the bulky child, there is no such necessity for a mechanism of the delivery of the comparatively small placenta as there is for the passage of the child's head. There is ample room and verge enough for the placenta passing in any way. But the natural mechanism claims respect as the *natural* mechanism, and, moreover, it presents obvious advantages over any other mechanism. Just as the child's head passes through the pelvis so as to dilate the passages as little as may be, or in the manner demanding least expenditure of force, so also does the placenta. It comes edgeways. If it came inverted, or transeversely doubled up, or folded into a cup shape, we should have a body passing that required at least twice as much space as is required if it passes edgeways, and only longitudinally folded. But this is not the only advantage of the natural mechanism.

If the placenta is expelled as Baudelocque describes, and as Schultze depicts, then a loss deserving the name of a hemorrhage is almost as necessary as it is certainly a generally described accompaniment of the process of the expulsion of the placenta. For the placenta has a certain amount of rigidity, and its folding on itself and the forcing of it into a cup-like shape cannot be effected without a hollow space being offered for the reception of blood, or indeed without a certain force being exerted to produce the folding and a vacuum, which force will also tend to draw blood into the said hollow from the open uterine sinuses which were in apposition to the part folded. Baudelocque's descriptions and Schultze's drawings, while they do not give what is natural, yet do indicate a mechanism of which they justly make considerable hemorrhage a necessary or nearly necessary part. It would be easy to show great if not insuperable, difficulties in the way of accepting Baudelocque's description of this mechanism. It is enough for me to assert that it does not exist as a fact, save as a rare exception to the ordinary process. I say no more, because I have no intention of entering here on the subject of hemorrhage during the detachment and expulsion of the placenta.

According to Baudelocque and Schultze, the folding is always on the uterine surface. But this is far from being the case. According to Lemser, it is more frequent than folding upon the fœtal surface. In my drawing I have represented the placenta, with a view to pictorial facilities, as folded upon its fœtal surface.

If the placenta comes edgeways, its uterine surface glides along the surface of the uterus; its foldings, parallel to the length of the maternal passages, are well squeezed together, and little space is offered for the reception of blood flowing from uterine sinuses. The uterine wall keeps close to the folded placenta. The uterus contracts, forces the placenta downwards, and at last its body is nearly globular and empty. There is no hemorrhage worthy of the name. Hemorrhage, when it does occur, is not demonstrated to take place according to the description of Baudelocque or the plate of Schultze; and I believe these gentlemen do not give the correct account of it. Authors too frequently, I may say almost invariably, describe too great an amount of hemorrhage as part of this natural process. I admit that the frequency of some hemorrhage is a strong argument in favour of this proceeding. But I believe that interference, which, though common, is frequently injudicious, is occasionally the cause of this hemorrhage, which is, therefore, in such circumstances, unjustly laid to the account of the natural mechanism. It is far from uncommon to observe labours in which there is no hemorrhage, in which not an ounce of blood is lost during delivery, there being only enough to smear the uterine surface of the placenta with a very thin layer. This absence of hemorrhage I regard as the natural state, and in this I suppose all obstetricians will join me, at least if I introduce the element of desirableness as an indication of naturalness. Such absence of hemorrhage depends on the adoption of what I describe as the natural mechanism. The presence of hemorrhage is a part of the erroneously described natural mechanism, and to me this presence is one proof of the erroneousness of the description.

But although the mechanism of Baudelocque and the picture of Schultze do not give the natural process, they indicate a state of matters which is frequently observed after the separation of the placenta. Schultze's second drawing is an admirable representation of what takes place frequently, perhaps generally, when any considerable force is used to deliver the placenta by traction of the cord. Then indeed, truly, the placenta is inverted, and its edge puckered up purse-like. The insertion of the cord comes first, as is so frequently represented in woodcuts. The placenta is transverely bent on itself, and puckered up; hemorrhage flows to fill up the partial vacuum which is thus

produced. The inverted mass forms a firm plug, closely filling the vagina. Traction on this plug is exactly like traction on the piston of a pump. If hemorrhage does not naturally take place to fill up the void which tends to be formed beyond the placenta, then it is powerfully attracted and induced by the piston-like action of the placenta pulled by the cord. The interior of the uterus, already scarified by the separation of the placenta, requires but this pulling at the cord to be effectively cupped.

From all this there follows the very valuable corollary, that in practice the third stage of labour should be left to nature, and that, when interference is required, the natural mechanism of the birth of the placenta should be as closely imitated as circumstances admit.

I shall conclude with a quotation from Cazeaux, which shows that this practical view has not been altogether neglected:—"'When the placenta is partially engaged in the orifice (of the womb) by a portion of its periphery, this plan,' says M. Guillemot, 'ought to be somewhat modified; for in this presentation, the root of the umbilical cord, instead of corresponding to the cervix, is higher up in the uterine cavity; and hence, if the operator resorts to traction, the centre of the placenta will have a tendency to enter the orifice, and thus add its bulk to the disk already engaged there. Such a disposition sometimes constitutes an obstacle to the further delivery of this mass; but it is surmounted by making some moderate tractions, not on the cord itself, but rather on the part previously engaged, by applying two fingers on its surfaces.' We have," adds Cazeaux, "had numerous opportunities of testing the practical utility of M. Guillemot's advice."—*Edin. Med. Jour.*, *April* 1871, *p.* 899.

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## 84.—CASES OF PUERPERAL CONVULSIONS TREATED WITHOUT BLEEDING.

By Dr. J. J. PHILLIPS, Assistant Obstetric Physician to Guy's Hospital.

[The plan of treatment pursued by Dr. Phillips has been in all cases the use of chloroform, without venesection. Whilst Chailly-Honoré (1859) relates that he saved eighteen out of the nineteen cases which he treated with chloroform, and Professor Braun, of Vienna, did not meet with a single death in sixteen cases treated by the same means; older records show a mortality of half, or at best, one out of three. It is undoubted that, at the present time, venesection is more frequently employed than any other remedy, in the treatment of the disease. Notwithstanding that, Brown-Sequard and others state it as a fact, that at the outset of an epileptic attack the great nervous

centres of an animal subjected to experiment become pale, instead of presenting signs of congestion.]

It will only be necessary to refer to the teaching of Dr. Radcliffe, Dr. Hughlings Jackson and others, that the mass of evidence goes to show that spasm is a sign of enfeeblement of nervous matter. In the words of Jackson, "All clinical evidence points to this one general conclusion that nerve tissue is enfeebled in convulsions. Whether it be itself primarily in fault, or whether it suffers from want of blood, is poisoned by bad blood, or torn by cerebral hemorrhage, there can, I think, be little doubt that it is enfeebled." And again, "I have no faith whatever that convulsions depend on any increase of nutritive changes that we can arrest by taking blood out of the system."

The most plausible argument which has been adduced in favour of bleeding in puerperal convulsions appears to be that it acts beneficially by relieving the circulation, lessening secondary congestion, and thus preventing injury, especially to the brain and spinal cord. In severe and frequently repeated convulsions death may of course threaten at any time by asphyxia, and the pulmonary congestion may be beneficially lessened by a judicious bleeding. There appears to be, however, in the minds of many, a belief that venesection is in some way specially suitable for the convulsions of puerperal women; whereas the puerperal state itself furnishes a strong argument in favour either of its omission or of the use of the lancet with a sparing hand.

Moreover it appears to me that the necessity of the employment of bleeding for the object just mentioned will diminish greatly in proportion to the frequency of the administration of chloroform at an early period of the attack. We have in chloroform an agent capable at least of materially controlling the severity of the paroxysms. The experience of several British and continental physicians is strongly in its favour, and the following cases, so far as they go, tend to prove its efficacy.

It is to be remembered that long before the discovery of the intimate connection of puerperal eclampsia with albuminuria, the frequency of serous inflammation, especially of the peritoneum, in such cases had been very generally observed; and at a time when venesection was considered necessary to control the progress of such inflammation, this formed an additional reason for its performance. But since the serous inflammation probably bears an intimate relation to the kidney affection, there does not seem to be here any argument in favour of bloodletting; while the great liability to puerperal troubles of a toxæmic character after severe hemorrhage during labour, indicates the danger of depletion, at least in many patients, at such a time.

While we cannot consider bleeding as a remedy for puerperal convulsions, neither can we claim for chloroform, in the majority of cases with which we meet, any direct curative power. The theory that its beneficial effects are due to the temporary diabetes which it produces, and to the influence of this state in rendering innocuous the retained urea, is at least very doubtful. Not only will the convulsions frequently recur at first if the chloroform be altogether discontinued, but a perusal of the following cases shows that sometimes a convulsion will occur while the patient is under the influence of the anæsthetic. Generally, however, whether the convulsions result from uterine or vaginal irritation, or from the poisoned state of the blood, they are controlled by chloroform.

In thus either preventing the recurrence or in checking the violence of the paroxysms, chloroform is of great value in preventing or diminishing the secondary congestions and the exhaustion, which are universally admitted as the dangers to be apprehended from convulsions. In many cases also chloroform renders unnecessary any interference with the course of labour. With such an agent at command it seems to me doubtful whether it be advisable in the first stage of labour to resort to any mechanical means for dilating the neck of the womb; but if it be determined to accelerate labour at this stage, then chloroform renders harmless a proceeding which, notwithstanding its perfection as at present accomplished, could hardly fail to be accompanied by a temporary aggravation of the convulsions.

Moreover it is of great service in convulsions occurring during the second stage, when it is often expedient to deliver cautiously by instrumental aid. Thus to both the medical and the obstetrical treatment of these convulsions chloroform is equally applicable.

When the patient has already suffered from frequent convulsions before being brought under the influence of the anæsthetic, and great pulmonary congestion has resulted, as indicated by the continued lividity of the face and the quick respiration (a condition, however, in which the pulse may be frequent and irregular), the withdrawal of a few ounces of blood may probably be beneficial before administering chloroform. This, however, was not done in any of the cases here recorded.

The recommendation of Professor Braun should also be remembered, that if chloroform be not given in time to cut short a certain paroxysm, as much atmospheric air as possible should be allowed to enter the lungs during the convulsive attack and the coma immediately succeeding.

I know of no other precautions specially applicable; and I may say that I have never witnessed any untoward symptoms from cloroform in these cases, though I have had it administered

in some instances continuously for several hours, and sometimes when the patient was already in a deep comatose condition between the attacks.

*Case* 1.—A patient of the Guy's Charity, aged 24, who had had two miscarriages, but no labour at term. At five a.m. on November 23rd, 1868, during the first stage of labour, she first complained of headache and in twenty minutes' time she had a severe convulsion, which was repeated in half an hour. She was seen by me between six and seven o'clock, when a third violent paroxysm occurred. Body much distorted, face very livid, with foam at the mouth, teeth clenched. She was partly conscious between the attacks. The pulse was full and beat 112 per minute. A fourth fit of a severe strangling character soon happened, and lasted longer than either of the previous ones. There was no œdema. The bowels, it was stated, had not been relieved for two days. A drop of croton oil was given, and the patient placed under the influence of chloroform, and kept so until ten minutes past ten. Once or twice, when allowed partially to come to, twitchings of the facial muscles gave warning of a fit, but none occurred. At eight o'clock the os uteri was the size of a five shilling piece. At ten a.m. the uterus was acting well, and the head, evidently of a dead fœtus, was occupying the pelvic cavity. Labour progressed favourably, and the child was expelled at mid-day. The patient's urine was albuminous, but neither casts of tubes nor any blood could be detected. After delivery she was rather restless and wild in manner, and the pulse was 100 per minute. On the second day after labour the urine was free from albumen, and subsequently the patient's convalescence was uninterrupted.

[Several other interesting cases are given, and the author sums up with the following conclusions:—]

These cases are, of course, far too few in number to warrant conclusions being drawn from them alone. Evidence is, however, accumulating in favour of chloroform; and I venture to submit the following propositions, which are, I think, supported by theoretical considerations and practical experience.

That bleeding has no claim to be regarded as a remedy for puerperal convulsions; and that in the majority of cases at least, if seen at an early period of the attack, it is unnecessary.

That bleeding is often injurious, by predisposing to various puerperal ailments, by retarding convalescence, and sometimes by increasing the violence of the paroxysms. Also that the present diminished mortality is probably chiefly due to the less free depletion which is now practised.

That the chief reliance should be placed on chloroform, which prevents the recurrence, or diminishes the violence of the paroxysms.

That in mild cases it is sufficient to keep the patient very slightly under the influence of chloroform in the intervals, more being given when indications of a fit are seen; but that in severe cases the patient should be kept for a time uninterruptedly under its influence.

That if the convulsions have already produced much pulmonary congestion, it is beneficial to withdraw a few ounces of blood before administering chloroform; and that generally it is advisable to lessen the tendency to cerebral congestion by the application of cold to the head.

That it is rarely necessary to interfere with labour before the os uteri is dilated, or in those cases where the convulsions precede labour; but that it is usually expedient in the second stage to complete delivery, due regard being had to the condition of the uterus.—*Guy's Hospital Reports*, 1870-71, *p.* 343.

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## 85.—TWO CASES OF UTERINE TUMOURS, GIVING RISE TO HEMORRHAGE—TREATMENT BY PERCHLORIDE OF IRON INJECTIONS INTO THE UTERUS.

Under the care of Dr. MATTHEWS DUNCAN, at the Royal Infirmary, Edinburgh.

M. S., aged 38, was admitted to Ward 16, Royal Infirmary, Nov. 26, 1870. She is married, and has had three children, the last nearly two years ago. Until six weeks ago, she was a strong, healthy woman. About that time, after a hard day's work, she had a copious discharge of bloody fluid from the vagina, probably about three quarts in amount. This discharge continued in small quantities for a week, at the end of which time she had a second attack of bleeding from the vagina as violent as the first. Three days after this, a third time, bleeding from the genital passages set in, the blood lost being in greater quantity than on either of the former occasions. This last blood-loss left her in a state of extreme prostration. Since then, she has had a slight daily discharge of blood. When admitted into the Hospital, she appeared extremely pale and anæmic. On physical examination, the belly is found to be natural, soft, and resonant down to the pubis. *Per Vaginam.*—The finger introduced discovers nothing abnormal at first, but, on careful examination, the body of the uterus can be felt between the external and internal fingers, and feels enlarged, rounded, and not tender, about the size of a small egg. This swelling feels as if it affected the left side of the fundus chiefly. A probe passes into the uterus the natural length, and finds it to be movable. Speculum discovers in the vagina, trickling from the cervix, a small quantity of blood.

*Treatment.*—℞. Ext. ergot. liquid, ʒ j., twice daily; et syrupi ferri phosph., thirty drops thrice daily.

December 22. To-day, for the first time since admission, patient complains of a great blood-loss. On examination, the tumour described in the original examination feels larger. Uterus measures three inches and a half. One drachm of the liq. ferri perchloridi was injected into the uterus. Patient felt nothing.

23rd. After the injection yesterday, the discharge almost ceased.

26th. Since the 23rd, there has been a slight flow daily. To-day it is almost absent.

January 3, 1871. No bleeding since last report.

J. C. was admitted to Dr. Matthews Duncan's Ward, Royal Infirmary, Edinburgh, on December 19, 1870. She is 41 years of age, unmarried, and has had no children. She complains of a swelling in the left groin, increased flow at the monthly period, and of great pain during the first twenty-four hours of the discharge. She has suffered from this pain for about a year; it has been increasing gradually in intensity, and for the last two months has been very severe. The tumour first attracted her attention in the beginning of last October, when, stooping to pick up something, she felt the pressure of it. It has not, she thinks, increased in size since then. Up to the time that these symptoms set in, patient had enjoyed moderately good health, and had regularly followed her occupation as a domestic servant. She has had a copious menstrual discharge, however, for many years; for the last three years it has been very abundant.

*Physical Examination.*—In the hypogastric region is observed a prominent, hard tumour, approaching in size a five months' pregnancy, reaching to within an inch of the umbilicus, and presenting a surface nearly uniform, but not quite so. The tumour is quite movable, but not to a great extent. The uterine souffle, or something closely resembling it, can be heard, but not very distinctly. *Per Vaginam.*—The cervix uteri lies near the middle of the pelvis, and all around it is felt a dense hardness, somewhat lobulated. The cervix moves with every movement of the tumour. A probe passed into the cervix advances first backwards and then upwards. It enters about three and a half inches; cervix is small, and not softened.

*Treatment.*—December 30. Hemorrhage began to-day.

January 1. The bleeding is now severe. A drachm of the liquor ferri perchloridi was injected into the uterus this morning. No bleeding during the day, but to-night a little discharge has commenced to flow.

4th. Since last report there has been a little blood lost every day, but not approaching in quantity to what came away before the injection.

17th. Has had no bleeding since the 4th; lips do not appear so white; patient feels strong and well.

*Remarks.*—These two cases illustrate well that most dangerous symptom of fibrous tumour of the uterus—namely, hemorrhage. It might with propriety be called a bleeding disease, because, barring the obvious inconvenience of a tumour of large size, the subject of it generally suffers little from its presence when this is absent. In the instance of M. S., had not bleeding set in she would have been unaware that any unnatural condition whatever existed. Here so much blood had been lost that the woman was brought almost to death's-door, and another equally severe flooding would probably have killed her. This her Physician in the country attested. The treatment by means of which this condition can be most successfully combatted, is that which consists in the injection of perchloride of iron into the uterus when symptoms of the approach of a severe blood-flow set in—that is, when there exists profuse hemorrhage about the time the monthly period is expected.

Dr. Duncan's method of procedure is as follows:—After the length and direction of the uterus have been ascertained by means of the ordinary sound, a hollow one is passed into the organ. A syringe, composed of vulcanite, containing about a drachm of the liquor ferri perchloridi, is fitted closely into the orifice at the proximal end of the probe, and its contents are gently thrown into the womb. No pain is generally felt as a result of this injection, but a feeling of burning is sometimes complained of. Dr. Duncan is inclined to attribute this to the regurgitation of some of the iron into the vagina. Dr. Duncan has found this mode of treatment highly successful in many cases, and most so in those where the symptoms calling for it were most urgent.—*Med. Times & Gazette, Feb.* 11, 1871, *p.* 158.

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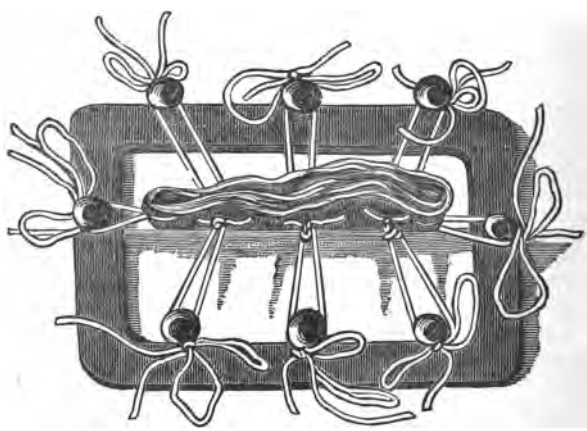
## 86.—A NEW INSTRUMENT FOR SECURING THE PEDICLE IN THE OPERATION OF OVARIOTOMY.

By Dr. GRAILY HEWITT, Professor of Midwifery in University College, and Physician Accoucheur to the Hospital.

Surrounded as this operation of ovariotomy is with difficulties of various kinds, one of the greatest of these is unquestionably the treatment of the pedicle: not merely the securing of the vessels enclosed therein in such a way as to prevent hemorrhage, but the dealing with it in such a manner as shall best conduce to the prevention of the pyæmic complications attendant or liable to be attendant on the healing of the wound.

For the purposes of the present paper, I assume that the best method of securing these two results consists in bringing the pedicle to the external wound, and there fixing it; considering as I do that, on the whole, this method is attended with less risk of secondary hemorrhage; while it does not appear to be, judging from the results—the magnificent results, they may be termed—of Mr. Spencer Wells's practice, more liable, at all events, than others to the pyæmic complications. Mr. Wells has largely employed, as is well known, the *clamp* in his operations. My experience, a far more limited one than Mr. Wells's, has made me acquainted with difficulties which seem inseparable from the present manner of clamping the pedicle, and which have given me no little trouble in some of the operations I have performed, and in which the clamp was used. The clamp, by its shape and form, hides from view the part of the wound immediately beneath it, and it is difficult to dress it and keep it clean and dry; while in cases of short pedicle it produces troublesome pressure on the edge of the wound, and leads occasionally to ulceration and sloughing from such undue pressure.



The accompanying wood-cut shews the instrument the actual size. A piece of leather is represented tied in the manner the pedicle is intended to be secured.

The newest form of clamp, though less liable to these objections, has another, which I have practically had experience of—viz., that it is liable to slip, and allow the pedicle to escape altogether.

The instrument and method I now suggest may be described as follows. A framework of steel, shaped something like a shoe-buckle, measuring two inches and a half by one inch and three-quarters; the piece of steel of which the framework is made being two-eighths of an inch wide and one-eighth of an inch thick, is provided with studs or buttons eight in number, three on two sides, and one on each of the other two sides. These buttons project three-eighths of an inch from the steel framework. It is to be used in the following manner. The pedicle, having been roughly trimmed, is perforated by a needle, armed with a double strong thread or whipcord ligature in two or three places, according to the width of the pedicle. It is then tied in segments; and the opposite ends are secured to the buttons of the framework above, one by one. The pedicle is now surrounded by the framework, while the cut edge of the pedicle is freely open to inspection and treatment in the centre; the tightness with which the ligatures are applied keeping the pedicle from slipping into the abdomen. In fact, the ligatures now act precisely as the tongue of a shoe-buckle.

The advantages of this method are, I think, sufficient to commend it. The wound can be more readily dressed; the pedicle is at all times accessible; the framework makes no pressure whatever on the edge of the wound at the point where the pedicle emerges; and the healing and covering in of the whole wound will be facilitated. There can be no question that dryness of the parts around the wound is most essential to the prevention of pyæmia; and it is exceedingly difficult to secure this with the ordinary clamp.

My first idea was to employ wire for the pedicle; but I find that strong twine is preferable, and infinitely more manageable. Messrs. Mayer and Meltzer have constructed the instruments herewith exhibited.—*Brit. Med. Journal, Oct.* 29, 1870, *p.* 458.

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## 87.—AN IMPROVED METHOD OF DIVIDING THE PEDICLE IN OVARIOTOMY.

By Dr. GEO. H. B. MACLEOD, Regius Professor of Surgery in the University of Glasgow, and Surgeon to the Glasgow Royal Infirmary.

The only step in the operation of ovariotomy to which the writer wishes at present to refer is the mode of dividing the pedicle, certainly not the least important or the least anxious which the surgeon has to consider.

Whatever method the operator employs to separate the ovarian tumour from its connection with the uterus, he must desire—

1st. To arrest bleeding effectually and permanently.

2nd. To injure as little as can be the stump which remains.

3rd. To separate the growth and treat the remnant of the pedicle in such a way as to occasion least chance of peritonitis.

4th. To return no dead tissue, or structures likely to decompose (and be a source of septic poisoning), within the abdominal cavity.

5th. To displace the uterus and its appendages as little as need be from their normal position.

6th and lastly, To close the abdominal cavity as quickly and as completely as possible.

It will in no small measure depend on how these ends are fulfilled what risk is run of hemorrhage, peritonitis, and shock —the three great sources of mortality after ovariotomy.

The different plans in use for securing the pedicle may be thus summarised:—

1. Tying it in one, two, or more portions with whipcord or silk, catgut or wire,—the pedicle being divided a short distance on the distal side of the constriction. The stump is either fixed in the wound; or (when any of the agents mentioned are used except wire) the ligatures are cut short and returned with the stump into the abdomen; or, being uncut, are fixed in the lower part of the wound, while the stump is dropped as before.

2. The pedicle is strongly constricted by a clamp, the growth cut away, and the stump fixed in the wound.

3. The division of the pedicle and the closure of its vessels may be achieved by the actual cautery, the stump being afterwards returned, or retained in the wound.

4. The peritoneal covering of the pedicle being first incised, and then the pedicle, after which each separate vessel is taken up and tied, and the stump restored into the abdomen or fixed in the wound.

5. The écraseur has been employed to separate the ovarian tumour.

6. The pedicle having been transfixed by a strong ligature, and a long pin being passed through it and both lips of the wound, the ligature has been firmly twisted round the pin.

A consideration of the whole circumstances of the operation must impress the surgeon with the extreme desirableness of restoring the stump of the pedicle at once into the abdomen, if that can be accomplished without greater risk than attends its fixture in the wound. What has hitherto prevented this replacement being carried out was the peril of hemorrhage and the injurious effects of returning tissue tending to decompose—the direct result of the mode in which the pedicle had been severed.

When we ligature the pedicle (whatever agent we employ for

the purpose) we necessarily kill a certain amount of tissue distal to the place of constriction, and to return this (and any portion, however limited, of the ligature) into the peritoneal cavity must be attended with manifest danger, though, happily, such danger does not always eventuate in catastrophe. If, again, we leave the ligatures long and drop the stump, we add for a time (often many days) to the risk of having decomposing tissue in the abdominal cavity the additional hazard of keeping that cavity open, and traversed by materials which cannot fail to imbibe septic elements, and serve as conductors of secretions into the depths of the pelvis.

If, again, by the clamp or other means we fix the stump in the wound we cannot quickly or effectually close the abdominal cavity, or ensure it against the entrance of decomposing secretions by the side of the pedicle; and, what is much worse, and in fact is well known to augment greatly the peril of the operation, the uterus is violently dragged from its place and twisted on itself. That the clamp is no complete safeguard against bleeding is amply testified by the records of ovariotomy, and that its use necessarily retards recovery may be conceded.

That the écraseur has failed effectually to command hemorrhage, and that it causes so great a mangling of the stump as to occasion subsequent fatal inflammation, is only what those who have used it most would anticipate.

The cautery has frequently failed as a hæmostatic agent in ovariotomy (the bleeding being sometimes early and occasionally late of appearing), and its action is so violent that much tissue is killed; and so, if the stump is returned, the objection made to the ligature cut short holds; if it is not restored, then the same difficulty which attends the use of the clamp presents itself.

Of the results obtained from the division of the pedicle and the separate deligation of its vessels, the writer knows very little, but from its rare employment in this country he supposes they could not have been promising. There would be the hazard of foreign bodies placed within the cavity, the greater risk of vessels subsequently bleeding which were not at first observed, and the occurrence of oozing which no ligatures can effectually command.

Regarding the pin and ligatures nothing need be said, as the same condemnation which applies to the ordinary modes of deligation applies to that method.

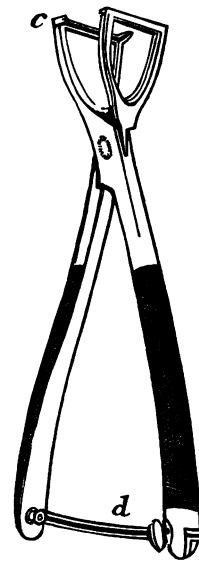
The plan which it is the object of this paper to submit to the profession seems to the author to obviate the chief objections before advanced against the methods at present in use, and though he has as yet only had one opportunity of submitting it to the test of practice, still it was so strikingly successful in

that instance that he is somewhat sanguine of its adaptability *to the majority* of cases. Short, thick pedicles may possibly not be amenable to the following plan, but experience can alone fully determine this.

The instrument here delineated (Fig. 1) is employed to grasp the pedicle firmly near its uterine end. The blades (*a*) are narrow, and, being male and female, they take a very firm hold; their apposition being retained by fixing the quadrant (*b*) with

FIG. 1.

FIG. 2.




the screw. The cyst is next cut away by dividing with a knife the extreme distal end of the pedicle, and the stump is caught, by means of the forceps here shown (Fig. 2), at a short distance from the part grasped by the instrument already spoken of. The form of the blades and the quadrant and screw enables this instrument also to take and keep a very firm hold. By slowly turning the forceps while the first instrument is kept at rest, the stump is twisted off close to the edge of the instrument by which it is held. Hardly any tissue remains external to the ...ng blades, and the amount of injury done is most trivial.

Possibly it would be better to incise the peritoneal covering of the pedicle before applying these instruments. The stump may now be returned with safety into the cavity of the abdomen, and the closure of the wound can be at once accomplished.

The case in which the writer employed this method was doubtless a very favourable one for such an experiment. Without entering into much detail, its leading features may be thus given:—

The patient was an unmarried healthy girl, nineteen years of age, who had suffered from an ovarian cyst of the right side for upwards of a year. The catamenia were scanty but regular before admission into hospital. She had been once tapped, about two months before ovariotomy was performed. At the time of the operation she measured thirty-nine inches in circumference at the level of the umbilicus. The uterus was central, movable, and of normal size. Fluctuation was marked and widespread. The abdominal parietes were unusually thick, and not apparently adherent to the cyst.

Ovariotomy was performed, in the presence of many medical men and students, on the 26th December. The chloroform was admirably managed by the house surgeon, Mr. Reid. No sickness followed. It is unnecessary to repeat the various steps of the operation. No bloodvessel was tied or twisted. There were no adhesions. The parent cyst was large, and enclosed many secondary cysts having various contents more or less glue-like. The whole weighed 21½ lb., not calculating much fluid imbibed by clothes, &c. Not a drop escaped into the abdomen. The pedicle was of medium length, and was treated as before described. More time was taken up in the twisting than was probably necessary, from the desire not to rupture the pedicle prematurely; yet it is doubtless requisite for security that the torsion should be slowly performed. Not a drop of blood escaped, and the stump was replaced into the abdomen, and that cavity closed in the usual way, but not so thoroughly as in future cases would be accomplished, simply because of the fear that, after reaction was established, it might have to be reopened to command bleeding.

Not a bad symptom followed. The day after the operation a too tight strip of plaster caused the patient some pain, which was easily allayed by removing the cause, and she had also, as is usual, some annoyance from flatulence; but, beyond this, she so rapidly recovered that, if it had not been for the dread of running unnecessary risk, she might have been sitting up in a week. She was allowed, however, to sit up on the eleventh day; and was altogether beyond restriction as to food, &c., in a fortnight. The exceeding thickness of the abdominal parietes and the paucity of sutures employed caused the superficial por-

tion of the wound to remain open longer than would otherwise occur. The exceedingly little general disturbance which followed, together with the perfect result, was not more gratifying than remarkable to all who watched the progress of the case. She was quite recovered about the time the clamp is found to separate in most cases.

The perfect arrest of all bleeding by the torsion was very striking, and, as it occurred in a strong healthy patient, it was a fair test of its power. Hardly a trace of injured tissue was left; immediate replacement was accomplished; the uterus and its appendages were restored to their normal position; and the abdominal cavity was effectually, quickly, and completely closed. In short, every operative requirement as regards the pedicle was fulfilled.

It is a question for further observation how far the simple grasp of the first instrument, if continued for a short time, might suffice to close the vessels of the pedicle without the torsion. If that succeeded, it could be applied below the peritoneal covering of the stump, and all tissue beyond it cut cleanly away.

In conclusion it may be stated that the idea carried out in the use of these instruments arose from seeing the apparatus employed by veterinary surgeons for castrating the horse lying in the shop of our most able hospital mechanist, Mr. Hilliard, and the strong conviction he expressed that some such appliance would be found an efficient hæmostatic agent in ovariotomy. The instruments before delineated were made by him for the author.—*Lancet, Jan.* 28, 1871, *p.* 108.

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## 88.—CASE OF OVARIOTOMY; TUMOUR TORN AWAY FROM THE PEDICLE WITHOUT HEMORRHAGE; NEITHER CLAMP NOR LIGATURE REQUIRED.

By RICHARD H. MEADE, Esq., Consulting-Surgeon to the Bradford Infirmary.

[The patient whose case is related in the following paper was a married woman, 48 years of age, who had had several children. The enlargement of the body commenced on the right side two years before. Fluctuation was very distinct, owing to the thinness of the fluid. As a preliminary measure, tapping was first performed, and after the removal of the fluid a freely moveable tumour, about as large as a child's head, could be distinctly made out, apparently springing from the right ovary. In five weeks the cyst had refilled, and it was determined to remove it.]

The patient being under the influence of chloroform, I made an incision about four inches long in the lower part of the linea

alba; carefully opened the peritoneum; and evacuated nearly two gallons of ascitic fluid. On enlarging the opening in the peritoneum to the same extent as the external wound, the ovarian tumour at once came into view. I now directed an assistant to compress the abdominal walls with his hands, one placed on each side, so as to press the edges of the wound backwards; while I endeavoured with my hands to draw the tumour partially through the opening. In doing this, the walls of several of the small cysts of which the tumour was principally composed (being very thin), were ruptured by the pressure of my fingers; and a considerable quantity of thick brown fluid, like dark-coloured linseed tea, escaped. The edges of the wound were so well compressed that none of this ovarian fluid was allowed to enter the peritoneal cavity. The tumour was now found to be firmly adherent to the free extremity of the great omentum; these adhesions were carefully and slowly torn through, and the whole mass was then easily drawn through the wound. It was now found to contain a good deal of heavy solid matter; and, on turning it over to examine its attachments, the pedicle, which was small and thin, to my dismay and annoyance at the time, gave way, and the tumour tore itself loose from its connexions. Fearing hemorrhage, I kept hold of the remains of the pedicle, but very little bleeding followed; and I could find no vessel requiring ligature.

I kept a large piece of softened sponge applied to the torn pedicle, as well as to the adherent portion of the omentum (which was much thickened) for fully a quarter of an hour, the abdominal walls being well covered up with hot flannels; I also had the actual cautery in readiness; but all oozing soon ceased; so I left the pedicle without any fastening, and proceeded to close the wound. I inserted a number of thick silk sutures greased with carbolic ointment (made by melting one part of carbolic acid with seven of lard) deeply through the sides of the wound; but *not* through the peritoneum. The line of incision was then covered with a piece of lint, thickly spread with the carbolic ointment; and over this a sheet of cotton-wool and a flannel bandage were applied.

The patient took chloroform very well, but retched slightly before the incision was closed. After she was placed in bed, and before the effects of the chloroform had quite gone off, I injected a sixth of a grain of hydrochlorate of morphia under the skin of the arm. The pulse was now rather feeble, but not quick, and she looked pallid and felt cold; but upon my seeing her again in an hour and a-half, I found her with a moderately full pulse of only 80 in the minute, and comfortably warm. There had been no more retching nor sickness, but she complained of abdominal pain: so I injected another sixth of a

grain of morphia. On leaving, I directed that she should swallow nothing but a little iced water, with a few drops of brandy in it, if she were faint.

On the following morning (fifteen hours after the operation) the pulse remained at 80; there was a plentiful secretion of urine, and she was very comfortable.

This patient recovered almost without a bad symptom; I let the sutures remain until the sixth day, when, on their removal, the wound was found to have united by the first intention. No suppuration occurred, except at one or two of the suture-holes, where a little ulceration had been caused by the pressure of the silk. On July 21st, less than a month after the performance of the operation, she was downstairs, and had been out of the house, able to walk without pain or inconvenience. She then looked rather pale and anæmic, but the wound was firmly healed; there was little or no abdominal tenderness, and no return of ascites; but a little deep-seated hardness remained on the right side. I heard through one of this patient's sisters that she continued well at the beginning of September.

Upon examination of the tumour after its removal, it was found that, in addition to the cysts filled with dark-coloured fluid, there also entered into its composition considerable masses of gelatinous colloid-looking matter, with others of a firmer consistence and brain-like appearance, containing clots of blood: showing, I fear, that the disease was of a malignant character.

It is difficult to account for the large quantity of ascitic fluid which was met with, unless there were some secondary deposits of cancerous matter on the peritoneum; but the history of the case would hardly lead to that supposition. The presence of a considerable amount of serous effusion in the peritoneal cavity has, however, one advantage in cases of ovariotomy; it seems to render the membrane less liable to take on acute inflammation, its delicate secreting surface having undergone some change; and after the removal of the ovarian disease it does not seem to be resecreted.

In some of the medical journals a case of ovariotomy has been reported (extracted from an American periodical), in which Dr. Julius F. Milner removed a very large ovarian tumour by enucleation, without using either clamp, ligature, or cautery, and without hemorrhage. When I commenced the above operation I had no idea of imitating his proceeding, but when the tumour (to my horror at the time) enucleated itself, and there appeared to be no bleeding from the pedicle, I determined to follow his example, and leave the torn surface unsecured; thinking that the risk from hemorrhage was less than that from inflammation from the presence of a foreign body in the peritoneal cavity. My case turned out successfully, and I think I should venture

to repeat the proceeding in some special cases; for instance, where the pedicle does not appear to be very vascular, when the attempt might be made to tear the tumour gently from its connexions (in the same way as adhesions are generally separated); but a firm hold should be kept of the pedicle, so that it might easily be secured in case of bleeding.—*British Medical Journal, Nov.* 26, 1870, *p.* 577.

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## 89.—SOME CLINICAL OBSERVATIONS ON THE MALIGNANT DISEASES OF THE UTERUS.

By Dr. FORDYCE BARKER, Professor of Midwifery in the Bellevue Hospital Medical College, and Obstetric Physician to the Bellevue Hospital, New York.

The diagnostic characters of the malignant diseases of the neck of the uterus are now so clearly given by authors, and so readily made out, that I have nothing to add on this point. I heartily concur with my friend, Prof. Thomas, in his statement that "for the proper differentiation of true malignant disease from neoplasms and the results of inflammation we are indebted to no one so much as to Dr. Henry Bennet, of London."

But little, however, has been written on the diagnosis of uterine cancer where the cervix is not involved. In fact, until within a few years past, the cervix has been regarded as almost invariably the primary seat of the disease, and this belief has been sanctioned by the authority of Ramsbotham, Sir Charles Clarke, Walshe, and Rokitansky. But it is now known that it not unfrequently occurs primarily in the cavity or in the parenchyma of the body of the uterus.

The diagnosis of malignant disease, when its primary seat is in the mucous membrane of the body, can generally be made out from the following symptoms, as summed up by Sir James Simpson: (1.) "The presence of a constant, profuse, and offensive discharge; (2.) Frequent, profuse, and intractable menorrhagia; (3.) The presence of a hard, rough, and irregular tumour in the cavity of the uterus, detected by the uterine sound, and felt by the finger after the dilatation of the cervix; (4.) Recognition under the microscope of the morbid structures, as evidence of cancer; (5.) The periodic recurrence of pains slight and intermittent, perhaps, at first, but soon reaching a high pitch of intensity, and then gradually passing off." But where the malignant disease is primarily interstitial in its seat, the diagnosis becomes much more difficult. Dr. Grailly Hewitt mentions the following symptoms as the leading points of diagnosis: "The patient presents symptoms of uterine cancer—a vaginal examination fails to give evidence of it—a careful ex-

amination of the uterus above the pubes reveals the existence of an irregular, or possibly, of a regular tumour, which may be of considerable size. This tumour, which in its physical character may resemble a fibrous tumour of the uterus, is distinguished from the latter by the presence of a more or less constant, offensive, and bloody discharge, and some of the constitutional symptoms of cancer." In cases where vaginal examination fails to detect disease of the cervix, I regard hemorrhage, coming on spontaneously some years after the climacteric, as almost conclusive evidence of malignant disease, for I have never known polypi or fibrous tumours to bleed, for the first time, any considerable period after menstruation has ceased.

*Treatment.*—In the cancroid diseases, I think the fact has been established, beyond controversy, that the disease may be successfully treated by ablation, if the operation be performed sufficiently early to extirpate the entire diseased structure. In the London *Medical Times and Gazette*, October, 1860, nearly ten years since, Mr. Hutchinson published a tabular statement of fourteen cases of "epithelial cancer of the female genitals," in which eleven recovered after excision. In two the disease recurred, and the patients died. In one the disease was too extensive for any operation. Since that publication, scattered through the medical periodicals, many other successful cases have been reported. Since 1856 I have ablated the cervix in eleven cases for this cause, nine of which have recovered and remain well. In one, death followed two days after the operation, from peritonitis, induced evidently by the imprudence of the patient. In the other, a patient of Dr. John W. Warner, the disease recurred a few months after, and the patient subsequently died.

In view of all these facts, it must be conceded that science has made great progress in successfully treating a class of diseases which, until quite recently, invariably resulted in a fatal termination. But the same success cannot as yet be claimed as to the other forms of malignant diseases of the uterus. And yet I cannot but feel that there is much to encourage us to hope that in the future we may yet attain a very much greater success in the treatment of this most terrible disease, than as yet has been dreamed of by the most sanguine of scientific men. In the first place, we are making rapid progress in our knowledge of the histology of the disease, and the laws of its development. In the next place, facts are accumulating which show that spontaneous recovery has occurred in some cases, and art may yet, in a certain percentage, secure the same result. Aran quotes one case from Estevenet, in which the cancerous uterus was separated entirely from its attachments, and expelled by the

woman in the effort of defecation, so that for some time there were hopes of her cure. Prof. Habit reports one case of spontaneous recovery in which the cancer involved both the uterus and the vagina. Virchow, Kiwish, Rokitansky, and Scanzoni, each report one case of spontaneous recovery from cancer of the uterus. Two cases of recovery have occurred in my own experience, unless there was some mistake in the diagnosis. As both of these cases were of great interest to me, I shall detain the Academy by a somewhat detailed history of each. The first, a married woman, aged 38, mother of four children, the youngest eight years of age, came to my clinic in 1852. She had previously been seen by the late Dr. Mott and had also been at the clinic of Professor Bedford, both of whom, as the patient said, told her that she had cancer of the womb. Whether this be true or not, I regarded it as such. She came to my clinic for nearly two years, during which time I applied the actual cautery some fifteen or sixteen times. I think, also, my friend, Prof. Charles Budd, who was then assisting me in my clinics, applied the cautery a few times. We were both of us greatly surprised at the result, as the disease seemed to manifestly decrease after each application, but it was not entirely gone, when she became pregnant. Dr. Budd attended her at the time of her confinement, when she gave birth to a living child. The first stage was completed before Dr. Budd saw her, and the second stage was less than an hour, a complete refutation of the *a priori* objection which has been urged against the actual cautery, viz., that it is liable to leave an indurated cicatrix, which would prevent dilatation of the cervix in the first stage of labour. Dr. Budd has informed me within a few months past that this patient is still alive and in good health. I know not whether, in the light of my accumulated experience since that time, I should regard that case as one of cancer or not, but I think there can be very little doubt as to the following—a case which, for a time, brought upon me some unmerited reproach, but which has subsequently given me a great deal of undeserved reputation. In 1856 the wife of a prominent lawyer and politician in Massachusetts came to this city for consultation. She had previously been under the care of the late Dr. Green, of Worcester, and been examined by one or two eminent physicians in Boston. Her husband informed me that they had called her disease "cancer of the womb." She was cachectic and very feeble, and for more than a year she had been suffering from severe and frequently recurring pains, repeated and profuse hemorrhage, with a constant, sanious, and extremely offensive discharge. The cervix was much enlarged, with a jagged, irregular surface, and the uterus was fixed and immovable in the pelvic cavity. My opinion as to the nature of the case coin-

cided with that which had been expressed by those who had seen her before, and, of course, my prognosis was most unfavourable. But with the hope of diminishing the profuse vaginal discharge and the extremely offensive odour, and that by accomplishing this I should make her journey home more comfortable, I decided to apply the acid nitrate of mercury freely to the ulcerated surface of the cervix.

Some two weeks afterwards, I received a letter of bitter reproach from her husband, in which he said that soon after her return, her mouth became very sore, and her physicians declared it to be the worst case of salivation that they had ever seen. Several of her teeth had come out, and the discharges from the vagina had increased enormously, with which she had thrown off masses of putrid and horribly offensive substances, so that it was difficult to remain in the room with her. The letter closed by saying: "Before you receive this my dear wife will have left me, and I shall always bear in my heart the feeling that you, by your experiments, have greatly added to her suffering and shortened her days." A few months afterwards I received a letter from him of quite a different tenor. She had quite recovered her health, and the disease was apparently cured. I have twice, within a few years, had the opportunity of making a vaginal examination of this lady. At the upper portion of the vagina a puckered cicatricial indentation can be felt and seen by the speculum, but nothing like the cervix or body of the uterus can be felt through the vaginal walls. Through the rectum I am able to detect what I suppose to be the remains of the body of the uterus. She is now 59 years of age, fat and stout and in robust health. It is an old adage that converted enemies become the most zealous of friends.

[Since this paper was read before the Academy of Medicine, a case has been published by Dr. Mettauer, of Virginia, which, in all essential features, resembles in a most remarkable degree the one related above. As these cases are, I believe, unique in medical literature, no apology is necessary for giving an abstract of Dr. Mettauer's case. "The subject of this case was a negress, aged about 35, married, but had never conceived, and who had for years suffered from uterine irregularities. . . . When examined per vaginam, the os and cervix, as well as a large portion of the body of the uterus, had disappeared entirely, as they could not be discovered by the most careful examination by the *toucher*, and the chasm remaining would have readily received an egg of moderate size. The examination caused some hemorrhage as well as pain, and the woman represented that bleeding was of frequent occurrence. The health of the patient was greatly impaired, as emaciation and debility had nearly rendered her helpless. So appalling was

the condition of this poor woman, that the writer, after a full examination, determined to discard the case, believing it utterly hopeless. The entreaties of the woman, however, induced him to alter his resolution, and, merely to quiet her distress, his consent to undertake to treat the case was yielded. After regulating the bowels, and the use for several days of soothing vaginal injections, it was determined to use the acid nitrate of mercury to the ulcerated uterus, which was applied by saturating cotton lint with it, and then conveying the tampon-like body thus medicated to the affected parts through the expanded three-bladed speculum. The lint was heavily charged with the nitrate, but not to supersaturation, and carefully pressed into the ulcer. Over this was laid a thick pledget of lint saturated with salt water, carefully packed round the first dossil. . . . In three days violent salivation came on, which continued over a month, during which there was no examination of the vagina or the ulcer. The lint came away during this time, but it was not known when. . . . The condition of the uterus was examined into the fifth week after the acid nitrate was applied, and most unexpectedly was found to have healed entirely, but no os, cervix, or appreciable body could be discovered, and all discharge had ceased. From this time the general health rapidly improved, and in two months the woman was apparently restored to sound health, her recovery being greatly promoted by the daily use of iodide of iron and a generous diet. She lived four years after this severe trial, enjoying good health most of the time, but finally died of some acute disease, of the nature of which the writer has never been informed."—*Contributions to Uterine Surgery, by John P. Mattauer, M.D., LL.D., Virginia. From Boston Med. and Surg. Journal, March* 10, 1870.]

If a young physician called upon to take charge of a case of uterine cancer should take up the most recent systematic works on gynæcology, as Byford, Thomas, Simpson, Hewitt, Courty, or Scanzoni, to ascertain what he could do to arrest the progress of the disease, to palliate symptoms, and to relieve suffering, I think the result would be a despondent impression as to any positive service he could render to his patient. Yet, in few diseases are the resources of art so manifest and so conclusive as in this, for we can most certainly improve nutrition, relieve pain, secure sleep, and arrest the sanious offensive discharges and hemorrhage, and thus notably prolong life and make it tolerable to the end. I have often thought that the feeling on the part of the physician that the case is hopeless, discourages the constant and assiduous care which may really be of great service to the patient.

It seems to me that, within a few years past, our therapeutic resources have greatly increased, and that we may confidently look in the future for additions of great value. I shall, therefore, presume to add a few words on the constitutional, the local, and the palliative treatment of this disease.

*Constitutional Treatment.*—The doctrine that cancer has a constitutional origin seems to have been accepted without question until quite recently; but it is now beginning to be doubted whether the disease has not a local origin which develops secondarily a blood contamination. This view has been put forth with great force by Mr. Moore, of Middlesex Hospital, London, and I think that those who have studied the subject are strongly inclined to accept his doctrine. If in the future it prove to be true, we may anticipate great results and new discoveries of therapeutic agents in the constitutional treatment of this disease. I shall only allude to such as I can speak of from personal experience in their use.

Arsenic has long been in use in the treatment of cancer, and a host of prominent names might be quoted who have expressed great confidence in its value in this class of cases. But with the great majority of even those who have devoted their attention especially to this class of diseases this remedy does not seem to hold an accepted position as one possessing any specific value; for many years, however, I have used it, and have regarded it as more useful than any other known remedy in this class of cases. My experience has convinced me that it possesses a decided power in improving the general condition of the patient as to flesh and strength, in diminishing the discharges, in mitigating pain, and in arresting the progress of the disease. Patients themselves become so convinced of its usefulness, that I have never known one willing to give it up, after having commenced its use. I prescribe it in small doses, three drops of Fowler's solution, to be taken always after eating. I have not been in the habit, until recently, of suggesting its use in the very advanced and hopeless cases, because I have expected but slight, if any, benefit in such cases, and I did not wish to jeopardize the reputation of a remedy which I esteem so highly. But in the past two years, I have often suggested it in these cases, and I believe it has always proved serviceable.

The Missisquoi water of St. Alban's, Vermont, has in some cases seemed to be of great benefit in arresting for a time the progress of the disease. In many, the use of these waters has seemed to produce no results. Many others, to whom I have suggested their use, have never reported to me, and it is therefore probable that the results in these cases were negative. But in a few instances, one of cancer of the breast, and four of

uterine cancer, the apparent effect has been more striking than any results that I have ever seen from any other agent. None were cured, but the ulcerated surface was cleaned off, as though it had been excised with the knife; the fetor, the discharge, and the pain ceased for some time, and the general health of the patient was greatly improved. One, a poor woman, whom I first saw in May, 1867, with ulcerated carcinoma, involving both the neck and the body of the uterus, and exceedingly feeble, emaciated, and broken down by the profuse and horrible offensive discharges and repeated hemorrhages, rapidly improved both in her local and general condition under the use of these waters. She is still alive, although the uterus is nearly destroyed, but her general condition is much better than when I first saw her. I will allude to another case, a patient of Dr. Sims. I first saw this lady with him in December, 1868. Dr. Sims informed me that, when he examined her in Paris, in August, the whole surface of the enlarged cervix was one mass of rough, ragged ulceration. When I examined her, the diseased surface was as smooth as if it had been excised with a knife, and there was no discharge and no odour, although she suffered from horrible pains. She died a few months subsequently, but Dr. Sims expresses the strong conviction (and I concur with him) that her life was prolonged by the use of the Missisquoi waters. Although the general sentiment of the profession does not coincide with mine on this point, I cannot but avow the belief that the therapeutic effects of these waters are eminently worthy of careful investigation.

Dr. F. A. Burrell has published a very interesting case of apparent cure of ulcerated scirrhous cancer of the breast. It was for months treated with an external application of a watery solution of carbolic acid and glycerine, without arresting its progress; but it began to diminish in size and soon cicatrized after the internal use of carbolic acid and sulphate of quinine was added to the treatment. A table-spoonful of the solution of sulphate of quinine (four grains to the ounce of water), with three drops of the solution of carbolic acid (two drops being equivalent to one grain of the crystals), was taken three times a day. It seems to me that such cases as these are of some weight in sustaining the theory of Mr. Moore.

*Local Treatment.*—In all cases where the cancroid disease is confined to the cervix and can be wholly removed, I think the question is now settled that amputation of the cervix should be performed. Most operators use the écraseur for this purpose, but I have always used the knife, because I think it leaves the excised surface in a much better condition for healthy cicatrization, and I have never had any trouble from hemorrhage

after the operation. I have several times seen the écraseur used for this purpose, and I cannot resist the feeling that the tissues must be more injured by this crushing process, and that they are left more liable to morbid development subsequently, than if the wound had been made by the clean cut of a knife. I am quite certain that the process of cicatrization is more rapid. It is hardly necessary for me to add that the operation should not be thought of if the disease exist above the vaginal attachment of the cervix, or involve the mucous membrane of the vagina, or where there is any evidence of its existence in any other part of the body.

*Cauterizations.*—There is a great difference of opinion among writers on this subject, as to the utility of cauterizations of the ulcerated surface of epithelioma. By some they are regarded as useless, if not positively injurious; as useless, because they do not reach and therefore cannot destroy the whole extent of the morbid tissue; and injurious, because they may influence deleteriously the adjacent structure and excite inflammation, thus favouring extension of the cancerous deposit, and hastening the progress of the disease. I have carefully studied the objections which have been urged, and I admit their full force, if cauterizations be indiscriminately and carelessly made. But under certain circumstances I have the strong conviction that they are often very useful in arresting the ulcerative process; in diminishing the profuse discharges, and thus saving the strength of the patient; in correcting the fetor, and thus lessening the chances of septic absorption. Where cases are seen at an early stage, and the ulcerated surface is small in extent, and the subjacent induration not profound, the progress of the disease has seemed to be manifestly retarded by their use. The principal agents which I have used for this purpose in the malignant diseases of the uterus are the actual cautery, the acid nitrate of mercury, and the chromic acid. In those cases where the malignant deposit has not extended to the adjacent organs, and particularly to the mucous membrane of the vagina, I use the actual cautery far more frequently than any other agent. My reasons for this preference are the following: It is a much more managable agent than any other, as it only acts upon the part with which it immediately comes in contact; the depth of cauterization can be perfectly regulated; no pain is produced by its use, either at the time or subsequently; and when the slough is thrown off no hemorrhage follows, as I have often seen occur when other agents are used.

I have chiefly used the acid nitrate of mercury in those cases where granulations sprout from the anterior of the cervix, which have a great tendency to bleed. But for some years past I

have become very cautious in its use, as in three instances I have caused profuse salivation by one application of this agent, and I have heard from medical friends of several other cases of like result. The susceptibility to this effect seems to be manifestly increased in a broken-down, cachectic system, and in such it is a consequence greatly to be deplored.

In cauliflower excresence, where the disease has extended beyond the reach of the knife, I generally make use of the chromic acid (100 grains to the ounce of water,) applying it very carefully with a pencil brush to the surface. Ordinarily its action is not followed by pain, but I should mention that in one case, six hours after the application of the chromic acid, severe pain came on, so that I was sent for in the night, and after using three hypodermic injections of morphia without relief, I was obliged to keep my patient under the full anæsthetic influence of chloroform for nearly four hours. A pretty severe attack of peritonitis followed, from which she entirely recovered in a few days. There was complete and apparently healthy cicatrization of the diseased surface, and for more than a year I deluded myself with the hope that the disease was cured. But when it again appeared its progress was very rapid, and she died in eight weeks after the first symptom of its recurrence. I have repeatedly made trial of various other caustics, such as the arsenical paste, chloride of zinc, &c., but without such results as lead me to substitute either of them for those above mentioned.

Dr. Routh, of London, has published two very interesting cases of apparent cure of epithelial cancer of the cervix uteri by the use of a strong spirituous solution of bromine (bromine, gr. v.—x. to fifty minims of spirits of wine). Since the publication of the paper by Dr. Routh, I have seen no case where the use of this agent would be admissible, but I am determined to try it when a proper case shall offer. It should be mentioned that Dr. Routh regards it as a powerful caustic, which is not unattended with danger to the patient if not carefully applied, and to the operator in its preparation. I have made great use of bromine in the treatment of the malignant diseases of the uterus, but in a different way and for a different purpose, which I shall allude to presently.

*Palliative Treatment.*—On this point I shall add but a few words. I have long been impressed with the belief that many practitioners err, in allowing the victims of this disease to suffer, by trusting to feeble and inefficient agents, such as hyoscyamus, belladonna, cannabis indica, &c., for the relief of pain, from the fear that the patient might become habituated to the use of opium, in short, an opium-eater. Now, I hold

that in such a disease as this we are bound to give our patients relief from pain if we can, no matter what the agent is, or how large the quantity or how long it may be continued. It has been said that a too early use of opium may lead to a habit which will injure the general health; but protracted pain, I believe, will injure the general health much more than any agent which may be necessary for its relief. If the milder agents will suffice for this, it is well; but there should be no hesitation, if they prove ineffectual, in at once resorting to the use of opium or any of its preparations which will best accomplish this. The special form of the agent which gives the most perfect immunity from pain, with the least reactionary evil, should be carefully ascertained by trial. In some, neither opium nor any of its preparations can be tolerated, and in these the chloral has proved an inestimable blessing. A patient that I now have, preferred to pass sleepless nights, tortured with pain, rather than endure the distressing nausea which followed a sufficient dose of any form of opium; and now she passes a blissful night of sleep by taking, on alternate nights, thirty grains of chloral. The effect is continued in a less degree the second night, but, if she takes it in any dose the second night, she suffers from both nausea and headache.

In some cases, there is great suffering from the extreme sensitiveness of the ulcerated surface, and this I have found best relieved by pessaries of iodoform, introduced at night into the vagina.

℞. Iodoform, gr. x.; butyri cacao, ʒ j.; glycerine, gtts. 5; M. One pessary.

These pessaries seem also to correct almost entirely the offensive odour of the discharges.

As lotions for arresting the discharges and removing the fetid odor, I have found the most useful to be a solution of bromine, or of permanganate of potash, or of carbolic acid. Where the cervix or any portion of the vagina is ulcerated, I am careful to direct that these lotions should not be forcibly projected against the diseased surface by any form of syringe, but that the vagina be filled by a syphon-tube from a vessel placed considerably above the patient. The "fountain syringe" accomplishes this result in a very convenient manner.—*American Journal of Obstetrics, Nov.* 1870, *p.* 519.

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## 90.—ON THE FUNCTION OF THE PERINEUM IN PROCIDENTIA UTERI.

By Dr. J. Matthews Duncan, Edinburgh.

[The following paper was read by Dr. Duncan before the Obstetrical Society of Edinburgh.]

The causes of prolapsus of the uterus are very imperfectly known. Various circumstances which predispose to it, or more directly produce it, are generally described. Among these are childbirth, diminution of the retentive power of the abdomen, hard work or violent exertion, or any other cause of ordinary hernia.

But of late years, especially since the extensive recommendation of operations upon the perineum as a means of cure of the morbid condition, procidentia has been ascribed to laceration of the perineum. Most modern authors on the diseases of women do more or less prominently adduce this laceration as an important cause.

My object in the present paper is, *first*, to show that laceration of the perineum is not, in any strict sense, a cause of prolapsus or procidentia; *second*, to point out what influence laceration of the perineum exerts in this disease; *third*, to show what is the value of restoration of the perineum.

1. *Laceration of the Perineum is not a cause of Prolapsus or Procidentia Uteri.*—No one, so far as I know, has explicitly ascribed to the perineum any part in the maintenance of the uterus in its natural position. This is very remarkable, considering the great combination of authorities that can be adduced, who, by ascribing procidentia to laceration of the perineum, thus evidently imply that it has some power to maintain the uterus in its natural site. To prove that it has no such power, I rely upon the following arguments:—

In several cases of complete destruction of the perineum, the laceration extending backwards through the whole of the sphincter ani, which have come under my notice, and on which I have operated, there has been no example of prolapsus of the uterus complicating the laceration; and the great majority of them has been observed in poor, hard-working women. The perfection of laceration has been present, and its influence has been supplemented by those of childbirth and hard work, and, notwithstanding, prolapsus has in these cases not taken place.

I have seen a large procidentia in a young girl, who had never menstruated, whose perineum was entire, and whose hymen was not ruptured at its posterior part. Cases of this kind are not extremely rare. The perineum is perfect, and yet the accident occurs.

Numerous cases have come under my notice, in virgins and in sterile women, where the integrity of the perineum did not prevent procidentia.

In the majority of the numerous cases of procidentia which come under my notice, the perineum is either entire, or not more injured than it is in the great mass of women who have borne a child.

I have no doubt that if, by way of experiment, the perineum was cut through in a healthy woman, no tendency to prolapsus would be thereby produced.

In judging of the state of the perineum in cases of procidentia, it is necessary to guard against a great source of delusion. In all there is, at first sight, an appearance of destruction of the perineum. Its antero-posterior dimension is always greatly curtailed, and this even in cases where it is perfectly entire, and where the hymen can be seen to be only injured and changed, not divided. The large bulky procidentia passing on the anterior margin of the perineum distends it, softens it, and pushes it back towards the anus, destroying its antero-posterior dimension, much as the body of the child does after the birth of the shoulders.

2. *The Influence of Laceration of the Perineum in Procidentia Uteri.*—While I have already given good reasons for believing that the perineum has nothing to do with the maintenance of the uterus in its natural position, and that laceration of it has no causative influence in the production of procidentia, there can yet be no doubt that laceration of the perineum favours or accelerates the occurrence of the procidentia in cases where the causes of this accident are in operation. The study of this point brings out the function of the perineum in this disease.

The uterus, morbidly propelled through the pelvis by the causes of prolapsus, comes at length to press upon the perineum, slightly distends it, advances along it, reaches the orifice of the vagina, protrudes through it, and is at last procident. It is evident, at a glance, that if this long course through the pelvis and over the perineum to the vaginal orifice is in any way shortened in its latter part, the sooner will the procidentia occur. To arrive at this stage of procidentia, the uterus has to go through a shorter course, and is therefore earlier procident. Now, laceration of the perineum abbreviates the latter part of the path of the uterus just described. It does not produce prolapsus—that is owing to quite other causes; but it abets such causes by removing difficulties which otherwise would have to be overcome. These difficulties lie not only in the length and resistance of the perineum, but also in the smallness and tightness of the vaginal orifice.

A comparison with some of the phenomena of natural labour appears to me to be both just and illustrative. Just as the perineum is not a source of any of the forces which concur to

produce the birth of the child, so it is in procidentia of the uterus. But though this is the case, its laceration makes the birth of the child more rapid and more easy; it secures a shortening of its course and an easy exit through the enlarged vaginal orifice. Just as a woman with laceration of the perineum will have the progress of a prolapsus to complete procidentia accelerated and facilitated in its latter part, so a woman in labour, who has previously had an extensively lacerated perineum, is liable to a precipitate birth.

3. *The Use of Restoration of the Perineum.*—I do not purpose here to describe the operation of renewing the perineum. I shall merely say that it is an operation I have frequently performed with much advantage. My object is to give what I believe is the correct theoretical statement of its use and value. There are other surgical appliances, especially the padded T bandage and the vaginal pessary, which are also of great use and value in the treatment of cases of procidentia. To these I shall have to refer.

It is a great mistake to suppose that a perineum, however renewed, removes any cause of prolapsus or procidentia. The causes of that displacement are all still present. The perineum is restored in order to resist the progress of the descending uterus. It may or may not succeed in doing so. This will depend on the force with which the uterus is propelled, and the force which the renewed perineum is capable of offering in resistance to it. In many cases the resistance offered by the new perineum is insufficient; it yields, and the uterus becomes again procident. The birth of a child may relax the renewed perineum without lacerating it, and thus, destroying its resisting power, may lead to a return of the procidentia; or, repeated childbearings may take place before the return of the procidentia.

Restoration of the perineum is not a cure of procidentia. It deserves in some sense this name only when its rigidity is such as to be sufficient effectually to oppose the progress of the uterus trying to force its way over it. This opposing power may be increased by the pressure exerted on it by the pad of a T bandage; or even a pessary may be further called in to assist in keeping back the propelled uterus. If one resistance to the advance of the uterus is not sufficient, another may be tried, or a third, or their forces may be variously combined to produce the desired result.

Here, again, the analogy of childbirth is very exact and illustrative. The child, like the prolapsing uterus, is propelled against the perineum. If the perineum is rigid, or if the propelling force is not great, the progress of the child may be

is a great recommendation for their use. My experience of them has been as yet limited to two cases, but in these they served me so well, and under somewhat trying circumstances, that I believe they will be found equal to any emergency.

*Case* 1.—B. C. was confined of her fifth child, in June, 1868, and was allowed, by an unqualified Practitioner, to remain thirty hours in acute labour. She was ultimately delivered by the long forceps by Mr. Secker, of Wakefield. I saw her with Mr. Secker, a few days afterwards, for an attack of metritis, and I gave it as my opinion that some sloughing would probably take place, and a fistula form, if she survived the process. Three weeks later I again saw her, and found that a large slough had separated from the lip of the uterus and anterior wall of the vagina, but the tissues were still in such a state that none but palliative measures could be employed. Six weeks later I again saw her, and found that, in spite of the use of the catheter, there remained two fistulæ—one, the larger of the two, being almost within the os uteri, and another, little more than a perforation, about three-quarters of an inch lower down. On September 24, I pared the edges of the lower opening, and inserted easily three stitches, close together, by means of the two needles. From the position and size of the fistula I could not, of course, see the points of the needles; but it will be easily understood that it is not necessary to see them. I removed the stitches on October 4, pared the edges of the other opening, and inserted five stitches. These were removed on the 14th, and a complete cure was effected. During these operations, I had the kind assistance of Messrs. Secker, Horsfall, and Whiteley.

*Case* 2.—M. S., aged 17, was confined of an illegitimate child in May, 1868, and is supposed to have been two or three days in labour. It is worthy of notice that many case of fistula occur in unmarried women, and it is due no doubt to their attempts to conceal their condition. After her labour, M. S. passed through what she called a fever, and, when she got well, found that her water constantly drained from her. After many months of suffering, she was placed under my care by the author of it, and I found that she had a small fistula immediately below the anterior lip of the os uteri. It would not admit a sound, and its position was only ascertained after the injection of milk into the bladder. Owing to the fact that the poor girl would not allow anyone to see her but myself, I had to operate on her without chloroform, and with the assistance of her mother alone, who held the duck-bill speculum for me. I pared the edges of the fistula, or rather cut it out in the midst

of a piece of mucous membrane, and inserted two stitches. These were removed in fourteen days, and a perfect cure resulted.

It is very pleasant to record successes, but failures are much more instructive. I therefore narrate (3) the case of ———, a patient of the late Dr. Whitworth, of Heckmondwike, near Bradford. She, too, had "happened a misfortune," as they say in Yorkshire, and had been several days in labour. The resulting sloughing must have been terrible, for she never menstruated after her confinement; and I never could determine the presence of a uterus by examination either by rectum or bladder. On passing the finger into what had been the vagina, and of which the opening seemed all that was left, the finger went at once into the remains of the bladder—a *cul-de-sac* about the size of a plover's egg, in which the papular orifices of the ureters could be felt. About three-quarters of an inch of the posterior wall and the whole of the urethra was left; nothing else but hard cicatricial tissue and an opening into the rectum three-quarters of an inch long could be determined. I must confess I was puzzled what to do; but, on careful consideration, I determined to try to close the rectal fistula, and if I succeeded in this, I should then have closed the orifice of the vulva, and made the whole remnants into a bladder. It is more than possible she would not have submitted to this, for, incredible almost as it may seem, I was given to understand that she still exercised some sort of sexual functions. What these were I am at a loss to understand. On December 8th, with the assistance of Drs. Sykes and Whitworth, I pared the edges of the rectal fistula and inserted four stitches. At my visit on the 15th, I found them all torn out, but from what cause I could not ascertain. I repeated the operation, however, and left strict injunctions that no solid food was to be given; half a grain of opium to be given every night, and the gut to be very gently washed out with warm water night and morning. On the 22nd I found that, while the opiate had been regularly administered, my other directions had not been attended to, and I was on that account not surprised to see masses of scybalæ forcing themselves through the much-enlarged fistula, and the whole thing a wretched failure. Dr. Whitworth was very anxious that I should try to take a flap from "some where;" but, even if had I tried Jobert's elytroplasty, I did not see where I was to attach the flap after having got it; nor had I any security that it would remain undisturbed. I therefore declined to interfere further in the case; and I had the satisfaction afterwards of learning that several others who tried their hands at the unthankful work were no more successful.—*Medical Times and Gazette, March* 25, 1871, *p.* 332.

## 92.—ON THE CURE OF THE CHRONIC PERFORATING ULCER OF THE BLADDER BY THE FORMATION OF AN ARTIFICIAL VESICO-VAGINAL FISTULA,

### AS PRACTISED BY THE LATE SIR J. Y. SIMPSON.

By LAWSON TAIT, Esq., Birmingham.

Much as Sir James Simpson did to advance the profession to which he was devoted, it is to be feared that there has died with him a great deal that would have been of infinite value to humanity had it been saved for us. Active as he was in recording, and indefatigable in making research in the art of healing, those who knew him can easily understand that there were numberless observations made and conclusions arrived at by him which have been left unpublished. It is with the view of saving a little of these that I venture to refer to two cases in the treatment of which I was associated with him, and in which the successful operation employed was a device original with him, and, as far as I have yet been able to discover, quite new in the annals of surgery. Of these cases I have no accurate and detailed notes. I may therefore be pardoned if I only briefly, and, as far as details go, perhaps rather succinctly, narrate the cases.

The first was the case of a young unmarried woman who had been in robust health until her bladder symptoms commenced. These consisted principally of intense pain round the neck of the bladder, bad at all times, but especially so during the act of micturition. A few minutes of comparative ease was obtained after the bladder was emptied; but the pain steadily increased as the viscus became distended, and she had again to pass through the terrible ordeal of its evacuation. This state of matters went on for many months, and every kind of sedative treatment was adopted, all kinds of injections into the bladder tried, and the ingenuity of her many medical attendants taxed to the utmost, without avail. The urine was always alkaline, but only slightly so; and it contained but a very small quantity of pus, with a trace of albumen. She at last was placed under the care of Sir James Simpson; and, as we came one day together from her bedside, he reasoned to the effect that the indication in her case was to put the bladder in a state of complete physiological rest; and to secure this it would be necessary to establish an artificial fistula. He carried his idea into execution, and with the most brilliant result; for the pain was at once relieved, and in a few weeks the ulcer was healed, the fistula closed, and the woman returned to her country home perfectly recovered.

The second case was that of a sempstress who, if I remember rightly, was a long time under the care of my friend Dr. War-

burton Begbie, and was transferred by him to Sir James's ward in the Edinburgh Royal Infirmary. All other treatment having failed, an artificial fistula was established, with a result equally satisfactory as that obtained in the other case.

The pathology of these cases is well illustrated in another which is now under my care, in the person of a lady who has come to me from a long distance to be the subject of this operation. She is a young married woman, and had been in perfect health until her marriage, five years ago. She has had no children, and has suffered for more than four years with symptoms perfectly similar to those described in the first case. The only relief she obtains is from the injection of a strong solution of nitrate of silver, and the administration of this is so painful that it has to be effected under chloroform, Under my direction a catheter has been introduced into the bladder in order to imitate the condition arrived at by the establishment of a fistula, but the instrument cannot be retained more than a few minutes; and I know of nothing short of the fistula which will cure her.

The performance of the operation is easy enough: it is effected by introducing a grooved staff along the urethra, and slitting-up the posterior fourth of the canal and about an inch of the posterior wall of the bladder. There is no difficulty in getting the fistula to close after the ulcer has healed; the difficulty is to get it to remain open long enough. The *rationale* of the operation is quite philosophical; but in principle it is unlike anything in surgery with which I am acquainted, except the establishment of an artificial fistula in the male perineum in order to promote the successful performance of an extensive urethro-plastic operation, which Sir Henry Thompson was kind enough to show me a few months ago.

The special form of ulceration by which these three women have been affected is referred to briefly by Rokitansky as a limited perforating ulcer; and, as I was once fortunate enough to be present at a post-mortem examination of a fourth case, in which the pain had caused death, I am in a position to verify that distinguished pathologist's views. In anatomical characters, as well as in semeiology, the ulcer closely resembles the perforating ulcer of the stomach; and Rokitansky tells us that in the bladder, as in the stomach, one of the means of the fatal issue is by complete perforation ending in peritonitis. It is somewhat curious that all of the four cases which have come under my own notice have been women, and comparatively young—circumstances which, together with others, remove it far from the ordinary catarrhal ulceration of the mucous membrane of the bladder. Of many post-mortem examinations of cases of the latter disease which I have made, I do not remem-

ber ever to have seen one in which the muscular coat was affected; the mucous coat seems to be invariably dissected cleanly off the subjacent tissue, and the ulceration is limited neither in extent nor locality. The perforating ulcer seems most frequently to exist at the neck of the bladder.

I have never met with or known of a case resembling these in a male patient. Were such a case to occur, should we be warranted in cutting him as if we wished to remove a stone from his bladder? I believe we should; for, if the disease is so terrible in a woman, what must it be in a man? and how much less likely the latter would be to derive benefit from any other form of treatment.

In surgical practice, as in almost everything else, it is extremely difficult to introduce anything really new, and it may prove that this treatment for the perforating ulcer of the bladder may not be so novel as I suppose it to be. It cannot, however, be very generally known; and if by making these two cases of Sir James's public I shall at any time be able to place in the hands of any of my brethren a successful means of treating a terrible disease, my object will be accomplished.—*Lancet*, *Nov.* 26, 1870, *p.* 738.

# ADDENDA.

## 93.—DR. RICHARDSON ON CHLORAL HYDRATE.

Before commencing a lecture lately delivered on Experimental and Practical Medicine, Dr. Richardson offered some observations on the subject of Hydrate of Chloral, to which we would call the attention of our readers. He said:—

My lecture to-day is on Suspended Animation, but I will ask you to allow me first to offer a note or two on another subject, at this moment of urgent importance. I refer to the administration of the hydrate of chloral. There have recently been two assured deaths from the hydrate. In the course of the past ten days, I have myself been consulted not fewer than three times on what have been considered dangers attending the administration of the hydrate, and I know generally that doubt and uneasiness prevail in the profession respecting the abuse as opposed to the use of this agent. I think it right, therefore, as I had much to do in introducing hydrate of chloral into medical practice in England, to answer a few of the questions that are most pressing from this place, where so many demonstrations of the action of the hydrate have been carried out.

1. *Is the practice of resorting to the use of hydrate of chloral, as a narcotic, in the absence of medical advice and direction, becoming a common practice among the people?* The answer to this question is strictly affirmative. The novelty of its administration and of proving its effects at an end, the hydrate is not at the present time used so largely by the medical profession as it was a few months ago, when its true place in the Materia Medica was less clearly defined. The sale of the hydrate of chloral to medical men is consequently considered as declining, while the general sale is, perhaps, increasing. Corresponding with this state of things we, in the profession, are becoming conversant with cases of what may not improperly be called "chloral drinking," and in which serious and singular symptoms are presented. Three classes of people specially resort to hydrate of chloral—viz., alcoholic devotees, who take the substance to counteract excess of alcohol and to relieve alcoholic delirium; sufferers from neuralgia and other painful chronic diseases, who find in the substance temporary relief from pain;

and persons having much mental worry, grief, or care, who, flying to it at first in order to obtain sleep, continue it until the occasional practice becomes a persistent habit. As an indication of the quantity of hydrate of chloral used in this country since its introduction here about a year and a half ago, I may state, incidentally, on what I have every reason to consider reliable authority, that one commercial house alone has supplied the English drug market with ten tons of the substance; three other houses have, it is supposed, supplied as much; so that fifty tons weight have been, on this calculation, sent out —an amount which, divided into grains, would yield over thirty-six millions of narcotic doses to England alone since August, 1869.

2. *What is a dangerous and what is a fatal single dose of hydrate of chloral?* The largest dose I have known to be taken is one hundred and twenty grains. This dose produced a prolonged and dangerous coma, but recovery ultimately followed. I think we may consider a hundred and twenty grains, as a maximum dose for an adult, dangerous, but not of necessity fatal. Beyond a hundred and twenty grains the danger increases, and a hundred and eighty grains may be considered a dose that would prove, in the majority of cases, positively fatal.

3. *What quantity of hydrate of chloral can be given with safety in divided doses during a stated period of time, say of twenty-four hours?* Judging from the physiological effects of hydrate of chloral in relation to dose, and to order of phenomena in relation to time, I should infer that the body cannot decompose and throw off the hydrate more rapidly than at the rate of from five to seven grains per hour. There will be difference according to age of person, the temperature to which the body is exposed after the dose has been taken, and the largeness of the dose, a small dose being disposed of quicker in proportion than a large one. But the variation is not such as to alter materially the rate of action from the estimate given. I should consider, consequently, that a hundred and twenty grains, administered, even in divided doses, in twenty-four hours, would be the safe limit of administration. In the treatment of tetanus this proportion has been exceeded, but not, I think, to the safety of the patient; for the fact that the hydrate of chloral overcomes or reduces the spasm is no safeguard against its own poisonous effects. From what I know, I conclude that the hydrate of chloral can be given to the extent of overcoming the severest spasm; but if the dose be carried too far, with the determination of removing spasm at all risks, the success may easily be bought at the expense of a fatal narcotism from the remedy.

4. *Does the frequent administration of hydrate of chloral lessen or increase the danger of the administration?* On this question I am forced to state that the frequent administration of the hydrate of chloral, though it may suggest greater confidence in it on the part of those who take it, increases the danger from an excessive dose. Hydrate of chloral differs from opium in this respect. Opium produces chronic symptoms peculiar to itself, but the dose may be steadily increased without any immediate danger from the increase. Hydrate of chloral cannot be used in this accumulative way without danger. In a word, although a person may become habituated to hydrate of chloral, there is a limitation of the quantity to be taken safely, which limitation is not materially modified by persistence in the habit of taking it, but rather the reverse.

After discussing three other questions, relating to the symptoms and pathological changes incident to the habitual use of chloral hydrate, to the chemical tests for the hydrate in cases where it has caused death, and to the post-mortem distinctions in instances of chronic poisoning by the hydrate and by chloroform, Dr. Richardson, concluded by observing that, as the world was indebted to the profession of medicine for the benefits derivable from the hydrate of chloral, it behoved the members of the profession to use their influence in protecting the public from an agent which, under improper use, might be turned from its good purpose to positive evil.—*Lancet, Feb.* 11, 1871, *p.* 209.

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## 94.—DANGEROUS AND FATAL RESULTS FROM THE USE OF HYDRATE OF CHLORAL.

By Dr. H. W. Fuller, Senior Physician to St. George's Hospital.

[In the following paper some cases are recorded in which moderate doses of chloral were productive of dangerous and even fatal results. They show that more caution should be taken in its administration than is usually done.]

On Feb. 9th, 1870, J. S. was admitted under my care into St. George's Hospital, suffering from slight anasarca and bronchitis connected with chronic Bright's disease. He was restless and nervous and unable to sleep, and therefore, after he had been some days in the hospital, and was exhausted for want of sleep, I ordered the chloral draught of the hospital (containing thirty grains of chloral) to be taken at bedtime. Soon after he had taken it he jumped up in bed, clutched at his heart, and complained that the medicine produced a sense of burning. In the course of a few minutes he became violently delirious, and though after a time the delirium subsided. so much depression

ensued that Dr. Jones, our resident officer, had great difficulty in sustaining his heart's action. Gradually, however, the heart recovered itself, the pulse returned at the wrist, and in a few hours he was out of danger.

Having just read M. Liebreich's assertion that hydrate of chloral, when in contact with an alkali, is transformed into chloroform and formic acid, it occurred to me that the extraordinary results witnessed in my patient's case might be attributable to an alkaline condition of the stomach, whereby the chloral was at once converted into chloroform, and thus induced the symptoms which were observed. I therefore determined to try it once again, taking care on this occasion to guard against such an occurrence by administering the chloral in combination with a full dose of acid. The result, however, was precisely the same as on the first occasion. Again there was the same sense of burning and oppression at the chest, followed first by violent excitement and delirium, and subsequently by collapse with failure of the heart's action; and on this occasion, as on the last, Dr. Jones was long doubtful whether the man would recover. I need not add that I did not make trial of a third dose, even experimentally.

From that time until the first day of the present year I did not meet with any case which led me to question the harmlessness of chloral. I had given it to hundreds of patients in doses varying from ten grains up to forty-five grains, and had been called in consultation to two patients, one of whom had harmlessly taken two drachms and a half, and the other three drachms, in the night prior to my visit. In some instances it failed as a hypnotic; in some it produced headache; and in others it gave rise to more or less excitement; but in none was it followed by any symptom calculated to cause alarm. On the 1st of last January, however, I was called in consultation to see a case in which thirty grains of the hydrate of chloral proved fatal. The patient, a young lady aged twenty, who was previously in fair health, complained on Dec. 29th of constipation and other symptoms of stomach derangement, for which her medical attendant administered a pill at night followed by an aperient draught in the morning. On the 30th the bowels acted, and she was relieved; but she passed a restless night, and on the 31st complained of uneasiness in the lower part of the abdomen, which was attributed to the approaching menstrual nisus. As she was very hysterical, a neighbouring practitioner was sent for early, and when he met the family medical attendant in the afternoon, they determined, as she was nervous and restless, and had obtained little or no sleep on the previous night, to give her thirty grains of chloral. She took the dose about 10 P.M. on the 31st, and almost immediately became much excited and

complained of pain in the chest. In about an hour the excitement passed off, and she fell asleep and slept heavily all night. In the morning she was sleeping so heavily, and looked so pale, that the family became alarmed, and sent for the gentleman who had seen her the previous day. When he arrived she was very pale and breathing heavily—a sort of deep sighing respiration; there was no pulse at the wrist, and her extremities were rather cold. It was impossible to rouse her in the slightest degree. He gave her stimulants and applied warmth to the extremities, and gradually the pulse returned at the wrist, though at the best it was only just perceptible. The family medical attendant subsequently met him in consultation, and together they did all that appeared to be expedient; but as everything failed to rouse her, or in any way to alter her condition, they asked me to meet them in consultation at 2 P.M.

When I saw the patient she was lying on her back, with her eyes closed, and breathing heavily, the respiration having a distinctly sighing character. She was very pale, and somewhat cold; the skin was dry; the pupils were large and dilated, but acted sluggishly under the influence of a strong light; the pulse was scarcely perceptible, but the heart was beating regularly, about 120 in a minute, and though its action was very feeble, its sounds were clear and its rhythm was normal. There was no distention of the abdomen—indeed, it was flat and soft; there was no contraction or rigidity or undue flaccidity of the limbs. It was impossible to rouse her in the slightest degree; but when fluid was put into her mouth she swallowed without much difficulty, so that she took a full-sized wineglassful of brandy-and-water in the course of ten minutes.

The indications for treatment being obviously to sustain the heart's action until the effect of the chloral had passed off, we determined to give her brandy and diffusible stimulants as far as possible by the mouth, and to supplement our efforts in that direction by repeated injections up the bowel of strong beef-tea and brandy. However, everything, proved unavailing. She continued in much the same condition until about nine o'clock the following morning, when she sank, without having exhibited the slightest consciousness or moved a muscle from the time she fell asleep on the evening of Dec. 31st.

Judging from what I have learned from various members of our profession, I believe that although fatal consequences may rarely follow a dose of thirty grains of the chloral hydrate, yet that unpleasant, if not dangerous, symptoms are not unfrequently experienced. Dr. Tuke informs me that in a man whom he saw suffering from the effects of intemperance thirty grains very nearly proved fatal, the symptoms of depression and failure of the heart's action being most alarming; and Mr.

Fred. Webb, of Maida-vale, has given me the particulars of another case in which an elderly man very nearly lost his life from the effects of thirty grains. The faintness, pallor, and depression of the heart's action were excessive, and for some time Mr. Webb was in doubt whether he would be able to sustain the pulsations of the heart until the effects of the chloral had passed.

Doubtless these cases are quite exceptional, and are met with only in about the same proportion as cases of death from the administration of chloroform. But the facts I have cited are sufficient to prove that such cases are more frequent than is commonly supposed. Further, they inculcate the necessity for caution in the administration of the drug; and they point to the conclusion that thirty grains form too large a dose for ordinary use, and especially for a patient in whom the effects of the chloral have not been previously tried. As a hypnotic in nervous restlessness, ten or fifteen grains ordinarily prove efficacious; and I have not seen or heard of any unpleasant symptoms following these doses. But the cases above recorded prove that larger doses, though usually harmless and often marvellously efficacious, are not unattended with risk; and now that the public are beginning to dose themselves with chloral, as they have done of late years with chlorodyne, this important fact cannot be too widely known.—*Lancet, March* 25, 1871, *p.* 403.

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## 95.—EXPERIENCES WITH THE HYDRATE OF CHLORAL IN CANCER.

By WEEDEN COOKE, Esq., Surgeon to the London Cancer Hospital.

Long experience of the frequent failures of newly projected remedies in cancer has made me slow to express publicly any opinion respecting the use of hydrate of chloral; but the results obtained in the cases in which I have employed it are so charming, so persistent, that, fortified by the published experience of others, I feel bound to add mine in confirmation of the excellent results obtained from the judicious use of this drug, and also a remark or two on the doses which will probably be found most efficient and innocuous. The cases I have to record are these: *Case* 1.—A poor fellow, aged about 50, has epithelioma running up the rectum and spreading itself externally about the anus. He has been in several hospitals. By the employment of opium and conium externally and internally the pain is abated, but his digestion, which is naturally good, is thereby much upset. He was put upon ten grains of hydrate of chloral

three times a day; since which he has been more free from pain, and the digestion is now not interfered with. *Case* 2.—In a case of advanced cancer of the uterus attended with much acute suffering, especially in the back, twenty grains of the hydrate of chloral always produce a comfortable night's rest and no morning sickness. *Case* 3.—A bad case of epithelioma of the ear extending to the scalp, attended with much pain, and the patient is subject to frequent attacks of gout. Ten grains of hydrate of chloral given three times a day. By this he is made perfectly comfortable: there is no sickness, he takes his food well, and the gout has subsided. *Case* 4.—A very bad case of epithelioma of cheek, with perforation, attended with much pain and difficulty in taking food. Sleeps well with twenty grains of chloral. No morning sickness; appetite good. *Case* 5.—I was recently called to see a very anæmic lady suffering from sloughing cancer of the right breast, attended with much sickness and severe pain—apparently dying. She has been restored to comfort and appetite by ten grains of chloral, with five minims of prussic acid, three times a day. *Case* 6.—A most painful case of psoriasis of the lips and fauces, the mucous membrane being quite destroyed, leaving a raw surface, a long time under treatment by various Surgeons, relieved entirely from pain by ten grains of chloral three times a day, enabling the patient to take food comfortably, which had not been the case for a long time. *Case* 7.—A bad case of cancer of the tongue; great pain and inconvenience in taking food. Much relieved and the facility of taking food greatly increased by ten grains of chloral three times a day. Sleeps well since taking the chloral. *Case* 8.—Another case of uterine cancer with great pain in back is made comfortable by ten grains of chloral three times a day. Pain in back gone, takes food well, and sleeps well. In one case of uterine cancer the patient prefers the injection of opium into the rectum as being more efficient in the relief of pain, and in another case of cancer of the breast, attended with severe bronchitis, there was a complaint of headache and some delirium at night, but I am inclined to think that the dose had been too large in this case—viz., thirty grains. Thus far, as a night draught, I have found twenty grains quite sufficient. But when pain is persistent the ten-grain dose three times a day gives the greatest satisfaction. There is no headache, no sickness, no loss of appetite, nothing to hinder the patient taking exercise, and, so far as the disease will permit, pursuing his usual avocation. Although many Practitioners have advocated larger doses, I am still of opinion, fortified by the results of trials in many more cases than those above recorded, that no advantage is obtained by much exceeding these limits. Occasionally the combination of morphia

has proved beneficial, but in matters of practical science, believing that unitarianism is the only safe method of inquiry, I would urge that in all cases requiring hypnotic or sedative remedies the hydrate of chloral should be used, unaided by any other similar drug, and, if it fails, to abandon it altogether. Tincture of orange-peel in water covers the rather nauseous taste very effectually.—*Med. Times and Gazette, Dec.* 31, 1870, *p.* 750.

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## 96.—CHLORAL HYDRATE.

By H. Fly Smith, Esq., Chepstow Villas, London.

[The following case is worthy of notice, as it shows how greatly the effect of a drug may be modified by the vehicle in which it is administered.]

My patient, a lady, has been taking thirty grains of chloral hydrate in flavoured syrup as a night draught for the past three months, with the effect of lulling neuralgic pain, so as to allow of pretty continued sleep. The influence of the drug has usually commenced about two hours after administration, and continued during the night without marked symptoms. Two nights ago I changed the vehicle (a strong syrup containing ten grains of chloral hydrate to one fluid drachm) for camphor water in the same proportion. The customary dose was taken, three fluid drachms by measure, in a wineglass of water. I happened to be in the room at the time, and was surprised by seeing the patient about fifteen minutes after taking the dose sink down on the floor (she was standing by the bedside at the time) in deep unconscious sleep, as though thoroughly under the influence of chloroform. I placed her on the bed. In about five minutes the respiration became a little troubled, and the teeth clenched, perhaps due to the smelling-salts and eau de cologne which I held to the nose. Gradually semi-consciousness returned, and after fifteen minutes the patient was sufficiently restored to sit up and to assist in finishing undressing herself for bed; but she was wholly unaware of what had occurred, and when in bed soon dozed off, but the sleep was much more ungenial than ordinarily. In the morning she complained that the chloral had not lulled the pain so effectually as usual, and that she felt more wearied. There was no nausea.

The chief points I wish to call attention to are, the sudden invasion of anæsthesia, and the danger of injury or of suffocation to the patient had she been alone; the fact that the dose was the same that had been taken for a long period, and therefore a dangerous result was the less to be apprehended. The cause of the unlooked-for occurrence was due to the vehicle,

which favoured more speedy absorption than the syrup. The whole effect of the drug seemed to be expended in a single explosion, instead of, as formerly, being spread over a considerable period of time. The more tardy action of opium and other poisons when the stomach is full is well known. It would seem that in like manner the action of the chloral may be greatly modified if the conditions under which it is used are but slightly modified.—*Lancet, March* 4, 1871, *p.* 328.

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## 97.—CHLORAL HYDRATE: ITS INCONVENIENCES AND DANGERS.

By Dr. J. CRICHTON BROWNE, Medical Director, West Riding Asylum, Wakefield.

[The tendency hitherto has been to exaggerate the merits of chloral. All diseases have been indiscriminately treated with it. A considerable experience of its use at the West Riding Asylum has shown Dr. Browne that there are not only inconveniences attending its use but sometimes not inconsiderable risks.]

When dwelling upon the evils of chloral, I would not be understood to overlook its real excellence. I am far from disputing its usefulness in that department of practice with which I am most familiar. I am persuaded that it has already proved an important adjuvant in the treatment of some mental diseases, and will take permanent rank as a valuable means of regulating nervous action. It is not my purpose here, however, to extol its usefulness, or to define the conditions for its employment. My object is to indicate some dangers which beset its administration, and to show that these are especially apt to be incurred wherever the nervous system is weak or disordered. From the proof of these propositions there will follow as inevitable corollaries that chloral ought never to be taken except under medical advice, and that it ought always to be prescribed with caution and judgment.

Soon after experiments with chloral were commenced in this asylum, in February, 1870, I noticed a singular tendency to flushing of the head and face in many of those patients who were subjected to its influence. It was no uncommon thing to find a pale anæmic patient, to whom chloral had been given, presenting at certain hours of the day a floridness of countenance which would have done credit to the rudest health. Of forty cases in which chloral was tried up to the month of June, and of which I possess notes, this blushing was remarked in nineteen, in greater or less degree; in a few

suffusing only the cheeks, but in a much larger number involving the brow, neck, and ears, and assuming a depth of colour altogether unusual in the natural process. In one case, which is characteristic of many, I find it reported that half an hour after fifteen grains of chloral had been taken, the face up to the roots of the hair and down to the ramus of the lower jaw was of a dull-scarlet colour, very persistent under pressure, most intense over the malar prominences and bridge of the nose, and thence shading off in every direction. The ears partook of the same colour, which was also scattered in blotches over the neck and chest, the lowest blotch being over the middle of the sternum, and the largest about the size of a florin. This singular flushed condition, which was associated with slight contraction of the pupils, injection of the conjunctivæ, and excitement of the circulation, continued for about an hour and then disappeared, during a paroxysm of sneezing and emotional perturbation, to recur after the next dose of chloral. Watchful observance satisfied me that this chloral-flushing rarely presented itself after a single or even several doses of the drug, but generally occurred where it had been taken regularly for some little time. When alcohol in any form was taken along with chloral, then its occurrence was much more certain; indeed, the combination of whisky with chloral in treatment came to be considered a sure method of producing it. Once fairly induced, it was not so easily got rid of. The chloral might be omitted, and still it would show itself, after meals, for a week or ten days, and for much longer if the use of chloral had been long continued. It was a source of annoyance to those patients who suffered from it and who were intelligent enough to express their sensations. They complained of burning heat in the face, of feeling "all of a glow," and often at the same time of a sense of giddiness, inability to walk straight or steadily, and confusion of thought.

In seeking for an explanation of this chloral-flushing, it of course suggests itself that there are several analogous states. We frequently meet persons who flush much after eating, or after sitting with their backs to the fire. We meet others who flush in a similar way after partaking alcohol; and it has been observed that malt-liquors are particularly prone to give this circumstantial evidence of their imbibition in those unaccustomed to their use. Some ladies suffer much from this flushing affliction. If they taste wine, "the sudden blush devours them, neck and brow." One lady of my acquaintance becomes so instantaneously crimson after the smallest modicum of wine, that she has been obliged to adopt total abstinence principles when dining out. Apprehension and trepidation have doubtless something to do with the flushing in her case, as she is exceed-

ingly sensitive about the ruddy foible, the disfigurement which it produces, and the inferences to which it might give rise in readers of the Saturday Review. But these are only pre-disponents after all, for the same thing occasionally happens in her own house in certain states of health. Now in all such cases the phenomena themselves, as well as the rapidity with which they follow the exhibition of their supposed causes, leave no reasonable doubt that they are essentially and directly of nervous origin. Brown Séquard's researches establish beyond doubt that flushing, such as we have been considering, must be attributed to temporary paralysis of the vaso-motor centres of the head and neck; while Lister's researches guide us to the conclusion that such vaso-motor paralysis is traceable to an inhibitory influence. Some peculiar or violent impression made upon the afferent nerves of the stomach and the minute ganglia with which they are connected originates an influence which is conveyed to the great sympathetic centres, where it becomes an embargo upon the control ordinarily exercised over the dimensions of the bloodvessels. Immediate dilatation of these vessels with afflux of blood follows in those parts where capillary movements are most habitual, and where there is constant exposure to the air. The range of operation of an inhibitory influence, such as that I have been referring to, will of course vary under various circumstances. It may be limited to a diminution or arrest of the vaso-motor functions of the sympathetic centres of the head and face; or it may diffuse its effects more widely, and this it will do in proportion to its intensity and frequency of repetition, and to the degree of enfeeblement or susceptibility of the nervous system generally. It may extend to the spinal cord, medulla oblongata, and neighbouring parts of the encephalon; and this it apparently did in those cases to which I have alluded in which, along with the flushing of the head and face, there was impairment of muscular co-ordination and giddiness. It may also spread its effects over the whole cutaneous surface, as was illustrated in a case under my care in which a few doses of chloral brought on a sharp attack of urticaria, from which the patient had never previously suffered.

On the morning of Nov. 20th, 1870, E. R., aged thirty, ward 32, was noticed, about an hour after a dose of chloral, to be much flushed, and to present over her whole body a diffuse inflammatory redness so closely resembling the smooth eruption of scarlatina that it was thought prudent to isolate her in the hospital for contagious diseases. Here more characteristic symptoms were shortly developed. A number of long pale elevations, or wheals, showed themselves on the legs, shoulders, and waist, while similar ones could be produced on other parts of the skin by scratching. At the same time, burning, stinging

sensations, and a feeling of tightness and hardness over the whole surface were complained of, along with wheezing respiration, sharp pains in the eyeballs, headache, and lassitude. A dose of compound rhubarb powder was administered, and in five hours thereafter (ten hours from the beginning of the attack) the skin had returned entirely to its normal state.

It is clear that in this case the chloral was the toxic cause of the urticaria. It acted just as various kinds of food are known to do in producing this cutaneous neurosis. It produced, firstly, general capillary hyperæmia, due to vaso-motor paralysis; secondly, spasm of the cutaneous muscular fibres, due to irritation of the motor nerves; and, thirdly, hyperæsthesia with modifications of sensory impressions, due to changes in the peripheral extremities of the afferent nerves. Its effects had advanced a stage upon that simple vaso-motor paralysis to which I first adverted.—*Lancet, April* 1, 1871, *p.* 440.

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98.—HYDRATE OF CHLORAL—ITS POSITION IN MEDICINE.

By R. Uniacke Ronayne, Esq., Youghal Infirmary.

An admirable advantage, incidental to the employment of chloral, is the non-suppression or retardation of natural secretions, the bowels, skin, and kidneys continue to perform their normal functions unaffected by it to any injurious degree. The only unpleasant effect produced on the healthy, alimentary system is the occasional slight dryness of the fauces at morning, which, however, a drink speedily removes, and in respect to the secretion of the kidneys, where we also find a slight deviation from this rule. Here, however, the disturbance seems equally trifling. The urine passed immediately after a chloral sleep reacts neutral and does not reduce the copper of Fehling's solution; but that voided some hours later is more dense and does reduce the copper of Fehling's solution, thus similating a temporary glucosuria; in other respects it is normal. I have never seen constipation caused by even its lengthened administration. On this subject, Dr. A. M. Adams speaks with confidence—"In head affections, where there is torpidity of the bowels and retention of urine, chloral does not, like opium, encourage this state of matters, but would seem, owing to some reflex action, whilst effectually overpowering undue nervous disturbance, to stimulate or assist the bladder and bowels to a more healthy action." This avoidance of any inclination to check the eliminative processes, causes chloral to be especially valuable in fevers where retrocession of these is sure to be attended by serious consequences. The administration of chloral is not followed by morning headache, nausea, nor disinclination

to receive food. This sick headache is one of the most annoying followers of opium, and its absence from the track of chloral is an inestimable benefit; yet, further, I have seen some cases in which similar affections (though not arising from any drug) have been effectually removed by its exhibition, returning on its remission, and again departing on its restoration. So far from giving rise to intolerance of food, the patient awakes bright, comfortable, and refreshed; free from lethargy of mind or body, and having generally a better desire for breakfast than previously felt. This appetite is a curious result of the use of chloral, and I have so often observed it, and noticed the patients themselves comment on it, that I cannot help associating it as a special attendant on chloral action. The craving for the morning meal which it produces cannot be accounted for by the good night's rest and the absence of pain alone, and I have been particularly struck with the numbers long deprived by cachexia of a taste for food, who voluntarily rejoiced in their newly acquired relish for breakfast. Action of the skin, so far as I have observed, seems little changed by it, if anything, I fancy it lessens the sweating of phthisis, by quieting the general excitement, and causing contraction of the capillaries. These appear to me the ordinary phenomena which accompany and follow the presence of chloral, but it is also capable of developing well marked particular effects. Amongst the most advantageous of these seem to be the decided control it exercises over the irritable cough of bronchitis and phthisis. In many of these cases, opium is highly objectionable; conium useful, but uncertain; and hydrocyanic acid often next to worthless, unless given in dangerous doses. Here, chloral quickly gives rest, and renders the harassing cough less frequent and harsh. Dr. Taylor, writing to Sir James Simpson of its benefit, in a case of congestive bronchitis with hæmoptysis, says—"As I contrast the distressed and audible breathing of last night with the tranquil sleep and improved state of the patient to-day, I cannot help concluding that, chloral has a *directly sedative effect on the whole respiratory surfaces.*"

Besides this, I have noticed other and important therapeutic actions result from its use, not the least useful of which, is its power of diminishing the quantity and improving the quality of the sputa in the above-mentioned chest diseases; in such cases it not only controls the severity and lessens the hardness and frequency of the cough, but it also reduces the actual amount of expectoration in a remarkable degree, sometimes cutting it down by one-half or more in a single night. In every case of this kind, I have achieved a similar result; this I consider a most valuable power, as by it a world of restless, exhausting misery is spared the already over-taxed weakness of the invalid,

and life is prolonged by the husbanding of such slight vital resources as he yet possesses. In asthma, the ability of chloral to avert and arrest spasm borders on the wonderful. In severe cases where the nocturnal paroxysms were excessive, keeping the wretched sufferer sitting up in bed gasping for breath, a timely dose taken on the approach of dyspnœa, has invariably warded off the attack, or, if it had already set in, relieved the spasm, permitted the recumbent posture to be resumed, and procured a quiet night's rest. Those to whom I have given it have derived more benefit from its use than from stramonium, ethereal tincture of lobelia, or other antispasmodics, especially favoured in such cases; it has been spoken highly of in pertussis, but on this point I cannot treat with experience. Dr. A. M. Adams, however, thinks highly of its value, and writes—"Where the cough is unusually severe, and likely in consequence to lead to other complications, it will prove a most valuable remedy—about six grains given only at bed time will in most cases ensure a comparatively quiet night." Perhaps, the most peculiar result which I have undoubtedly obtained from chloral administration, although I am at a loss to account for it physiologically, is its efficiency in arresting the disastrous colliquative diarrhœa of phthisis. Although, as I before observed, its habitual use does not tend to the production of constipation, yet I have several times by its administration stayed the vital torrent where mist. cretæ c. opio, vegetable astringents and sulphuric acid with opium had hopelessly failed. Of course, there are limits to such action, and I should have little faith in its good offices where ulceration had actually taken place, but, in the stage of tubercular irritations which precedes the breaking down of the mesenteric deposits, I have experienced decided success by its aid alone. Chloral may be used with benefit in many forms of irritable stomach. It almost always calms that excitable organ, and even holds somewhat in check the obstinate vomiting of organic disease. I have been able by its means to procure many hours' rest, and tolerance of food for an ulcerated stomach, and I know of no other preparation for which I could say as much. It is possible that it might prove equally useful in the obstinate vomiting of pregnancy, and even become a respectable adjunct to the marine surgeons' nostrums against sea sickness. In various forms of irritability of the heart, as well as in valvular diseases of that organ, I have used chloral with satisfactory results. Although M. Bouchut says, "it increases arterial tension," I have found it relieve fluttering, and by checking the disposition to irritability, allay irregular action. In valvular disease, it appears also to do good by steadying the organ down to its work, and thus relieving laboured over action. Sir James Simpson has called attention to one most valuable

property of chloral, viz.,—that of lessening the pains of labour without restraining contractions of the uterus. He observes,—"I have found the parturient uterus to go on contracting regularly and strongly when the patient was so deeply asleep under chloral, as to be only very imperfectly awakened up with the expulsive efforts of labour." Here, then, we find a much more convenient and safer companion in the lying-in chamber than chloroform, which latter can hardly be properly administered without the aid of a second medical attendant, a thing seldom attainable even in respectable private practice, and completely beyond the reach of the ordinary dispensary physician.

There is one disease of formidable aspect, about which, in connection with chloral, some controversy has arisen, I allude to the Sphynx of Medicine—traumatic tetanus. There are some very important cases of recovery on record where this agent has been credited with the cure; and that of M. Verneuil, in the Lancet, April 16th, 1870, seems the most convincing. On the other hand we have statements equally reliable, charging it with complete inutility to meet such straits; but as one swallow does not make a summer, neither can one or two cases for or against establish a rule. The truth may lie on both sides. All forms of tetanus are as surely not to be cured by any one drug, or combination of drugs, as that no hand could raise a Tower of Babel. Whilst on the other side many of the failures may have fallen unjustly on the shoulders of chloral, as all practitioners meet occasionally with cases of constitutional idiosyncracies, where susceptibility to the action of some particular medicine is found wanting; and it is possible that amongst the unsuccessful ones such may be found. I have met but one peculiar case of this nature, where a man suffering from chronic rheumatic arthritis, took large quantities of chloral without its producing the smallest effect; whilst a patient in the next bed to him spent his nights in a pleasant oblivion derived from one-sixth the dose. I fancy the former's insusceptibility arose from a hyper-acidity of the system to which his disease made him liable. But should more extended trial prove chloral to have no power over tetanus, I still think that should not make us undervalue what it has actually done in the broad fields of medicine and surgery. Amongst the other ailments in which chloral has found favour, I may mention hysteralgia, dysmenorrhœa, hysteria, pleurodynia, gastralgia, and cancer. In irritable bladder and chronic cystitis, Sir James Simpson has seen "it give the patient much longer and more perfect rest than large doses of opium." Liebreich speaks well of its utility in rheumatism, convulsive coughs, acute mania, *delirium tremens*, and as a sedative after surgical operations; and Bouchut considers it the most prompt and efficacious remedy in intense

chorea, where the life of the patient is threatened. It is as well to mention that I have seldom found it necessary to prescribe chloral in higher doses than from fifteen to forty grains, and that it is neither an accumulative medicine nor one which must necessarily be increased in quantity on repetition—the same amount which relieved pain, or produced a soporific action, this day month does the same to-day. Now, as to its toxic effects, it seems that the functions acted on by chloral are attacked in this order:—1. The cerebral; 2. The voluntary muscular; 3. The respiratory; 4. The heart; and it is only when given in doses sufficiently large to induce a depressing effect on the last, that any threatening or fatal result is to be feared; and since the ultimate action of chloral and chloroform are similar, only that of the former is infinitesimally slower than that of the latter, it follows that poisoning by chloral must be more than proportionately rare.

Having thus endeavoured to illustrate the various uses to which chloral may be applied, it only now remains for me to express the conviction which a lengthened and impartial, even jealous, trial of chloral has forced upon me, namely—that it is by far the surest, safest, most efficacious, and least objectionable hypnotic which we possess; that it is far superior to opium in all the qualities which they hold in common, and in the absence of all the ill effects which the latter is capable of exercising, and that with the exception of such cases as rupture of the uterus, peritonitis, and such like, where opium's very vices are its best commendation, it is capable of completely supplanting that drug.—*Med. Press and Circular, Dec.* 28, 1870, *p.* 518.

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## 99.—HYDRATE OF CHLORAL AS AN ANÆSTHETIC IN LABOUR.

By Dr. W. J. C. Du Hamel, of Baltimore.

At 4 o'clock P.M., September, 1870, I was called to see a lady in labour with her second child. The previous labour, three years ago, had been a tedious one, and the physician in attendance had finally to deliver her with the forceps. I found her in active labour, the os uteri dilating; and a few hours afterwards the os had become well dilated and the head presented; pains active. A few hours later very little progress had been made though the pains were still vigorous, and she now insisted on the instruments being used saying she *could not be delivered* otherwise. I declined to do so, but gave her twelve grain doses of hydrate of chloral every hour for *three hours*. It proved to be a most excellent anæsthetic in this case, and free from the dangerous effects of chloroform. After the *second dose* there was

considerable *relaxation* of the parts, and in four hours after the first dose she was delivered of a fine child without the use of instruments, and with *very little pain*. I regard the chloral as a valuable agent in severe and tedious labours.—*American Journal of Med. Sciences, Oct.* 1870, *p.* 574.

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## 100.—ON NITROUS OXIDE GAS AND ITS USE AS AN ANÆSTHETIC.

By Dr. MACLAREN, Carlisle.

[The position of nitrous oxide gas as an anæsthetic is now thoroughly established and its use rapidly extending. The gas is prepared by decomposing nitrate of ammonia with heat. If the heat employed is too great there is evolution of nitrous acid, and other disagreeable oxides of nitrogen.]

Nitrous oxide is a colourless gas having a very faint odour and a slightly sweetish taste. Its specific gravity is 1·525. It supports combustion, but in a minor degree to pure oxygen. The test of its purity is that a match lighted and then blown out should have the red ember brilliantly re-ignited on being plunged into it. The phenomena usually observed in the human subject when the gas is inhaled, are as follows: when it has been inspired two or three times there is an increase in the force and frequency of the pulse. In about twenty seconds, if the breathing has been steady and regular to begin with, it is also noticed to increase in frequency. This, however, from the commencement is often irregular, shallow, and rapid, from the nervous condition of the patient. In about thirty seconds the patient's colour begins to turn livid. When the gas has been continued for about one minute the pulse is almost invariably noticed to fall in force and frequency, the breathing is often laboured, sometimes stertorous, though I have several times seen cases where it became feebler until the expiring valve was hardly raised; it was then either the case that the patient was completely anæsthetized, or, if not, the breathing commenced to improve in character. In the majority of cases, in one minute and twenty seconds the patient is over. This is known by different signs,—the one which is least open to mistake is a sudden change in the patient's appearance, which is difficult to describe, but readily recognised; another is a nervous twitching of the hands. It is not by any means necessary to carry the administration the length of this in every case, but when it is present for a few seconds the patient is quite insensible. In one case to which I gave the gas, this muscular twitching affected the whole body, resembling almost an epileptic convulsion; the patient was quite unconscious. The

same dependence cannot be placed on the insensibility of the conjunctiva to touch as in chloroform cases. For from reflex action patients will close the eye on having it touched when they are quite insensible to pain, and, on the other hand, they will keep the eye steadily open sometimes when not unconscious. As in one of my early cases, where a patient complained that he felt the whole operation, I told him I could not understand how that came about, for when I touched his eye he gave no signs of feeling it. "Oh," he irreverently replied, "I felt you touch the eye, but I kept it open as I only thought it was part of the performance." It occasionally happens about the period of the removal of the face-piece that the pulse intermits a beat or two, and I have repeatedly seen the breathing stop, during a period equal to four or five respirations, at the time when the operation was commenced. It is very curious, and a fact which I have not seen noted by others, that the hearing remains acute frequently after the other senses are in abeyance; thus patients are able to repeat remarks made in their presence after they are apparently unconscious, and one young lady was able to repeat a hasty remark made by the operator, Mr. Warwick Hele, when a tooth broke under the forceps, although she did not feel its subsequent removal, and the observation was made in so low a voice that I did not catch what it was, though I was standing beside him. There often exists considerable muscular rigidity during the later stages of the administration; so in dental cases, before the apparatus is placed on the face a plug is fixed on the teeth to keep the mouth open, as some difficulty might be experienced in effecting this afterwards, and much valuable time be lost. These plugs are made of vulcanite, with the faces so cut as to catch on the cusps or between the teeth. I have administered the gas myself upwards of forty times, I cannot tell you exactly how often, for of my earlier cases I have kept no record: all the cases have been for the extraction of teeth or fangs. Since we used the time indicator mentioned above, Mr. Warwick Hele and myself have carefully registered the cases. They amount to thirty-seven; in seventeen of these he gave the gas, in the rest I gave it. The time taken to produce insensibility was as follows:—

| | | | |
|---|---|---|---|
| Up to | 1 m. | inclusive, | 9 |
| ,, | 1 m. 20 sec. | ,, | 17 |
| ,, | 1 m. 40 sec. | ,, | 5 |
| ,, | 2 m. 0 sec. | ,, | 5 |
| ,, | 2 m. 20 sec. | ,, | 1 |

The shortest time was forty-five seconds; the longest, two minutes thirteen seconds. The greatest amount of work done has been four extractions of teeth or fangs, which has happened

in several cases. It has never happened in my experience that a patient was restless or excited while the gas was being given; in a few cases with children it has been necessary to hold the hands to prevent them catching the apparatus, but there is never anything resembling the struggling so common during the excited stage of chloroform. Recovery is usually rapid and complete, the patient waking up as from a sleep. But it does sometimes happen, usually in young ladies between the ages of fourteen and twenty, that there is an intermediate stage between that of complete unconsciousness and recovery, when some excitement is exhibited, such as tossing about restlessly in the chair and weeping, they, on recovery, being generally very much surprised to find themselves so doing. Out of the above-mentioned cases we have noted seven to have been affected in this manner in a greater or less degree, three complained of slight giddiness or faintness, and one was a little sick. One patient, a boy, urinated, his father having declined to indulge his expressed wish to do it before the operation. In several cases the removal of teeth was felt, but unattended with pain. The longest period during which we have noted a patient to be insensible is one minute. The patient is always seated in the upright position, this being found not only to be the most convenient for the operator, but the safest for the patient, as it obviates the risk of the tongue falling back on the epiglottis, and the saliva flowing into the throat, which is apt to happen if the patient lies on the back.

It is a point of great practical importance not only to keep the room perfectly quiet during the administration, but also to leave the patients undisturbed until they thoroughly awake. From a neglect of this precaution, I believe, resulted the only case of considerable excitement that I have seen. It was my second case, and the patient during recovery exhibited all the symptoms described as attending the exhibition of the nitrous oxide as laughing gas, *i.e.*, mixed with some air. In regard to the sensations experienced by patients, the majority are able only to say that they have been unconscious, some that they have been asleep and dreaming, the dreams being often pleasant, sometimes more like nightmare; and lastly, some complain that they have had considerable annoyance from rushing or singing noises in the head. One little girl was immensely pleased with it; she said that "she felt as if she was being tickled all over." Unconscious reflex screaming is not uncommon with children during the operation; but it does not seem to be connected with any particular form of dreaming. I have not been in the habit of ordering a stimulant before the gas is taken, as is commonly done with chloroform. In the only instance in which I sanctioned such a procedure, the patient, a

young, strong, but very nervous man, took one glass of undiluted brandy, and he suffered from headache for the rest of the day, which I thought might probably be due to the brandy, as I have never seen this sequent before or since in the same class of patient.

What is the physiology of the action of the gas? Sir Humphry Davy's researches led him to conclude that when breathed, and also when exposed to blood, a great part of it is absorbed, and that the residual gas is principally nitrogen. He believed, however, that the gas was not decomposed, but was absorbed only, and that the nitrogen was evolved from the blood. In regard to the residual gas being chiefly nitrogen Sir Humphrey seems to have been in error, for Dr. Hermann's observations, published in 1844, and quoted in the British Medical Journal of 18th April 1868, are as follows:—It neither enters into combination with, nor suffers changes from, nor produces changes in the blood, though readily soluble in it. A hundred volumes of blood at the temperature of the body will absorb somewhat less than sixty volumes of nitrous oxide; blood saturated with it shows no sign of change, the spectrum appearances are the same, the blood-corpuscles are unaltered, and the oxygen is not driven out. In the blood, and probably in the body, laughing gas suffers no change. It does not give up its oxygen for purposes of oxidation, and therefore gives rise to no free nitrogen; it leaves the body as it went in, pure and simple laughing gas. Hence it is of no respiratory use, and when mixed with a quantity of oxygen sufficient for the needs of the economy, has no more direct effect on respiration than has nitrogen or hydrogen.

Mr. Clover observes, "Nitrous oxide acts by preventing the oxidation of the nervous centres, principally by depriving the blood of its proper supply of free oxygen in the lungs. Although there is oxygen in the nitrous oxide, it is in a state of chemical combination with nitrogen, not free, and merely mixed with nitrogen as in the air. It is probable that the presence in the blood of nitrous oxide is exciting, so long as some oxygen remains, but as soon as the oxygenating property of the blood is lost, the functions of the nervous centres fail; and if fresh air were not soon admitted, their functions would cease altogether. The functions of the brain proper cease before those of the medulla oblongata; hence we have loss of consciousness before failure of breathing, and the functions of the medulla are abolished before those of the ganglia presiding over the action of the heart, and hence the heart continues to beat after the breathing has ceased." To the practical importance of this last paragraph I would direct your attention, for it shows that after the breathing has ceased, the patient may be safe so long as the heart pulsates.

Dr. Richardson believes that this agent acts simply by producing asphyxia, and is therefore a most dangerous anæsthetic.

I do not propose to indicate my adherence to any of these theories, nor do I intend to propound a new one of my own, believing as I do, that we have up to the present time been indulged with rather too many theories, and too few accurate observations or facts. And in leaving this part of the subject, I would simply quote to you the words of a wise man—Sir Humphry Davy: "It would be easy," he says, "to form theories referring the action of blood impregnated with nitrous oxide to its power of supplying the nervous and muscular fibre with such proportions of condensed nitrogen, oxygen, light or ethereal fluid, as enabled them more rapidly to pass through those changes which constitute their life; but such theories would be only collections of terms derived from known phenomena, and applied by loose analogies of language to unknown things." With some slight change of terms, would the remarks not as well suit our day as they did the beginning of the century?

Nitrous oxide has been chiefly used as an anæsthetic in dentistry, and seems eminently suited in every respect for this work. In general surgery, cases are recorded in which, by alternating it with air, insensibility has been kept up for eight minutes, twenty minutes (Dr. Marion Sims), sixteen minutes (Dr. Colton), and in this country, eight minutes thirty seconds (Dr. Begg), twenty minutes (Mr. Fox).

The advantages which I conceive it possesses as an anæsthetic are,—1st, Its safety. It has now been given for six years in America, and for two in this country, and a vast number of patients have been submitted to its influence, and there have been only two fatal cases recorded, both in America. In one of these the patient's lungs were found "riddled with tubercles" when a post-mortem examination was made; and in the other, the cork which was used as a plug to keep the mouth open, was found in the pharynx. In this respect, it compares favourably with chloroform and bichloride of methylene. 2nd, The absence of troublesome after-effects. It is only in very rare cases indeed that any complaint of sickness or faintness is made at any length of time after its use. I believe that the most prolonged inconvenience I have met with, was a headache, lasting the rest of the day; and patients have been always able to leave the operating chair and walk away within ten minutes of an operation. 3rd, The rapidity of administration. Time, often valuable time, is saved to all concerned. 4th, The position. This applies principally to dentistry, where the sitting is a much

more convenient position than the recumbent. 5th, there is nothing unpleasant in the smell or taste of the gas itself, and it can be inspired and expired freely; it does not cause any choking feeling or spasm of the glottis. Its disadvantages are,—1st, Rapidity of recovery from a single administation; insensibility does not last over one minute or one and a half minutes. 2nd, It is somewhat troublesome to administer; it requires close attention and a little practice to be able to give it with facility. 3rd, The apparatus for making and administering it is costly. Until about six months ago, I should have had to state that a serious objection to its use was the trouble of making it, and its inconvenient bulk for carriage. But the gas can now be procured compressed into the liquid form, and thus reduced to small compass. This, however, costs in London twice the price of the home-manufactured gas, and, I am told, makes so much noise when escaping as seriously to discompose patients. I have no doubt that in a short time the gas and apparatus will be much less costly, and that it will come into very general use in minor surgery. I believe that it will be habitually given in cases in which we never use an anæsthetic now, save under very exceptional circumstances, as in using the potential cautery, opening small abscesses, and in applying painful dressings, and that it will supersede chloroform in all operations not lasting over one minute. As to the cases in which it would be unsafe to give this anæsthetic, evidently those in which phthisis exists deserve the first place, for one of the deaths recorded above was attributed to this cause, and other cases are mentioned in which hæmoptysis occurred. From the frequency of a laboured or panting condition of the breathing during the later stages of administration, I think its use is to be avoided in the case of all diseases of the lungs causing serious impairment of the breathing, as chronic bronchitis, emphysema, and pleural effusion. Acute chest diseases I do not mention, as there is but little likelihood of the question of gas administration occurring during their course. In heart disease, it has been knowingly given without producing death, and with only temporary aggravation of symptoms. It has been given in such cases after the patient had been prohibited from taking chloroform. I do not think that such cases are free from danger, and I believe it should only be given in them when the result to be attained is such as to justify some risk, as where an operation was a necessity, and its performance without an anæsthetic seemed likely to endanger the patient's existence. Leaving out of consideration insanity, I do not think it would be advisable to give it where there was evidence of brain disease.—*Edinburgh Medical Journal, Jan.* 1871, *p.* 591.

## 101.—THE ADVANTAGES OF BICHLORIDE OF METHYLENE.

By CHARLES GAINE, Esq., Surgeon Dentist to the Royal United Hospital, Bath.

In April last I read a paper before the Bath and Bristol branch of the Bristol Medical Association on "Anæsthetics," and in it endeavoured to explain the advantages which, from repeated experiments, I thought the "bichloride of methylene" possessed over all others then known. Since that time I have administered it in a great number of operations, not only in my own special practice, but also for my colleague, Mr. Stockwell, at the Royal United Hospital, and in private practice, with the most marked success; notably in the following operations :—Lithotomy, reduction of dislocations, fistula in ano, recto-vesical fistula, resection of knee-joint, amputation of leg, excision of mamma, necrosis, cataract, iridectony, &c.—the oldest person operated upon being a woman, aged 70; the youngest, a child, aged 6 years. The shortest time in which anæsthesia was produced was twenty seconds, in a child, to sound for stone in the bladder; the longest, two minutes and a half, in a man, aged 24, for extraction of teeth—the shortest time which anæsthesia was maintained being forty seconds; the longest thirty-five minutes. The recovery has always been rapid and complete.

I first administered the bichloride of methylene in some half-dozen cases in 1868, but discontinued its use, because I found it less manageable than chloroform; the cause of my failure then, I have since discovered, was the admission of too much air in administering it. I subsequently employed the protoxide of nitrogen in my own practice, but the peculiar physiological phenomena exemplified in using this gas never impressed me so much in its favour as it has many others.

In January, 1870, I read a paper by Mr. Bader, of Guy's Hospital, published in the British Medical Journal of January 9, "On the Administration of Chloroform and other Anæsthetics." In this paper the cause of my former failure with the bichloride was fully explained. I therefore resolved to give it another trial; and to the courtesy of my friend Mr. W. R. Wood, of Carlisle House, Brighton, and Mr. Rendle, of Guy's Hospital, I am indebted for some valuable information as to the manner of administering it, and to the latter gentleman for a description of his inhaler also. This inhaler I have found to answer the purpose admirably. It consists of a hollow cylinder made of thick leather, about five inches long, and shaped at one end to fit the nose and chin, the other end having small holes punched in for the admission of air. It will be necessary to have three different sizes, as the efficacy of inducing rapid anæsthesia with

the bichloride of methylene depends on the inhaler accurately fitting the nose and chin, so as to prevent the admission of air at that end. A flannel bag hangs loosely within the cylinder, on which the methylene is sprinkled.

The following rules I have found of great importance, and the strict observance of them will tend greatly towards success in the administration of this agent:—It should never be administered without first preparing the patient. Abstaining from food and stimulants of every kind should be insisted on for from three to four hours before the time appointed for administering it; all garments should be loose. Auscultation not revealing any morbid condition to contraindicate its use, the methylene may be administered in the following manner:—The patient being either in the recumbent or semi-recumbent position, forty minims should be sprinkled on the flannel bag, and the inhaler then applied closely over the nose and mouth. A slight choking sensation will be first apparent, which passes off rapidly; if not, remove the inhaler for an instant. In from half a minute to a minute (seldom longer) anæsthesia of the eyeball is complete, though it will continue to roll about, showing more of the sclerotic than usual. The operation may then be commenced. Remove the inhaler entirely, until symptoms of returning consciousness are seen, when, if the operation is not completed, renew the inhalation with half a drachm more methylene; in a few seconds the inhaler may be again removed. In this manner, by proper attention to symptoms, a patient may be kept under its influence for any reasonable time. Should the operation be of short duration, from forty minims to a drachm of methylene will suffice for most cases. With two drachms and a half I have kept a patient under it for thirty-five minutes for resection of the knee-joint. I have not had a single case of vomiting, and two cases only that gave me the least uneasiness, and these had both departed from the rules laid down as to stimulants, &c.

To relate a number of cases would only occupy your valuable space for no useful end, but the following extracts from my note-book may not be uninteresting in comparing the effects of methylene with chloroform and protoxide of nitrogen on patients who had inhaled one or other of these agents before. In January last I was consulted by a clergyman who, some five years since, had on three different occasions inhaled chloroform for removal of teeth. My friend, Mr. Fowler, who administered it, told me that each time he had the greatest difficulty in getting him under the influence of it, and he was always prostrated for nearly a week after it. It was necessary to remove some particles of necrosed bone in the upper jaw, and puncture a deep-seated cyst. This was accomplished under the influence

of one drachm of methylene, and in three minutes from the commencement of the inhalation he was sufficiently conscious to converse with us, and inquire what had been done. He walked away a few minutes after, and has not subsequently felt the slightest ill effects.

A young lady had inhaled "protoxide of nitrogen" in London for tooth extraction. The first upper and lower molars on the left side were extracted; the corresponding teeth on the opposite side were broken in the attempt. It was necessary, for regulating purposes, to remove the stumps. She asked me to give her chloroform; she said she would not take gas again. Methylene was administered, and the stumps removed, and in five minutes she walked away without feeling any inconvenience.

In the case of a woman, from whom it was necessary to remove all the remaining teeth (twenty-two in number), I administered protoxide of nitrogen; she was so uproarious, and subsequently became so blue with stertorous breathing, that I was only enabled to remove three teeth. A week later, I removed the remaining nineteen under methylene at one sitting, seventy minims only of the bichloride being used. A quarter of of an hour afterwards she walked home, feeling no ill effects, further than the loss of her teeth. Very many similar examples could I mention, but, as I have said, no useful purpose will be served by so doing. Let it suffice that, in relating my success with the bichloride of methylene, I have confined myself entirely to what I have observed in administering this agent, and am alone responsible for any short-comings in this respect. It is equally useful in long as in short operations, and only requires to be more generally understood to give it precedence over all anæsthetics yet known.—*Medical Times and Gazette, Feb.* 25, 1871, *p.* 232.

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## 102.—BICHLORIDE OF METHYLENE (CHLORO-METHYL) IN GENERAL SURGERY.

By T. Spencer Wells, Esq., Surgeon in Ordinary to Her Majesty's Household, and Surgeon to the Samaritan Hospital.

[The following short article is part of a letter to the Editor of the Lancet in consequence of its having been stated that chloromethyl is not an agent "of very extensive utility," nor likely to supersede the use of chloroform in general surgery. Mr. Wells's experience is the very reverse of this.]

The first surgical operation in which chloromethyl was ever used was a case of ovariotomy, which I performed in October, 1867. It was administered by Dr. Richardson himself, and in his report to the British Association in 1868 he says:—"After

subjecting myself to the action of the vapour to the production of perfect insensibility, I ventured to administer it for surgical purposes on the 15th of October last. The sleep produced was of the simplest and gentlest character, and the operation, performed by Mr. Spencer Wells, which lasted thirty-five minutes, was quite painless."

This was my 229th case of ovariotomy. I have now done 417, and, with the exception of about 10, where, for some reason or other, chloroform was used, chloromethyl was the anæsthetic employed in every case, about 180 in number. In some 25 other cases of gastrotomy, and in more than 50 operations of more or less severity—such as herniotomy, amputation of the breast, removal of mammary or other tumours or of hemorrhoids, and plastic operations for the cure of vaginal fistula or ruptured perineum—chloromethyl has been administered for me either by Dr. Richardson himself or by my colleagues, Dr. Junker and Dr. Day. In very few of these operations was the condition of insensibility to pain maintained for less than five minutes. In a few it was kept up from forty-five minutes to an hour or more, and I should think the average would be about fifteen minutes. Yet I have never been at all uneasy in any one of these cases, more than 250 in number, either during the administration of the anæsthetic or from any subsequent ill-effects fairly referable to it. Whereas, with chloroform I never felt quite at ease; and, although I never lost a patient during operation, I have three times had to resort to artificial respiration, and I have very often seen patients suffer so much from chloroform-vomiting for many hours after operation, that the result has been imperilled, and in some cases a fatal result has been in a great measure due to the vomiting. It is quite true that chloromethyl has also "the disadvantage of causing nausea and occasional sickness;" but in my experience this is almost the rule with chloroform, whereas with chloromethyl it is certainly exceptional.

When I add that between April, 1870, and March, 1871, I had thirty-two successive cases of ovariotomy in private practice without one death, every patient having recovered, it must be admitted (as anæsthesia was complete in every case, not one patient having been conscious at any stage of the operation) that the anæsthetic employed is a good one. In some cases less than two drachms was used, and very rarely more than six drachms. Dr. Junker's apparatus was generally employed, and Mr. Krohne tells me that many practitioners on the continent, in America, and in different parts of our own country, who have ordered it from him after seeing it in my practice, have used it without difficulty, and have been well pleased with the results.—*Lancet, April* 29, 1871, *p.* 591.

## 103.—ON THE EFFECTS OF THE PROLONGED USE OF MORPHIA BY SUBCUTANEOUS INJECTION.

By Dr. FRANCIS E. ANSTIE, Senior Assistant Physician to Westminster Hospital.

[There is no doubt that the morphia syringe has been greatly abused. This has arisen to some extent from a non-appreciation of the distinction between a stimulant and a narcotic dose of morphia, as explained in this paper.]

That there is a special state of chronic narcotism induced by the continued repetition of *large* hypodermic doses of morphia is a fact familiar to many physicians. It differs strikingly from that which results from the persistent abuse of opium or morphia taken by the mouth, in the slighter effects which it produces on consciousness, and especially in the fact that it usually scarcely impairs, if indeed it does not decidedly increase, the activity of appetite and digestion; hence it is far more consistent with the active performance of the duties of life, and with the maintenance of general bodily nutrition; and so far it must be considered as a much smaller evil than the constitutional habit induced by the old-fashioned forms of opium-excess. But it is quite as fatally effective as the latter in weaving a chain of habit from which the patient can either not escape at all, or can do so only by great efforts involving the prolonged endurance of much distress. It is certain, then, that the physician can only be justified by very special circumstances in allowing such a condition to be set up in any patient under his charge. Fortunately I am enabled to say, from a large experience, that it is only in rare instances that we are reduced to this necessity.

There are, in fact, three degrees of the action of morphia, hypodermically injected, which differ from each other in essential particulars: (1) that induced by small purely stimulant doses, given only once or twice, or repeated at intervals of twelve to twenty-four hours for a shorter or longer time; (2) somewhat larger doses, gradually reached, and repeated with some frequency over a considerable period; (3) large doses, repeated daily or oftener, requiring to be continuously augmented, and often continued, of necessity, throughout the remainder of the patient's life. The first two of these degrees of action are the only ones which, in the great majority of cases, the physician ought to employ.

1. The hypodermic use of purely stimulant doses of morphia is of wide application, and of a value which it is difficult to exaggerate. In the intense pains of the early stages of acute serous and fibrous inflammations; in the early stages of neuralgias; in the insomnia and delirium of many adynamic fevers:

in phagedænic ulceration; in the dyspepsia of nervous irritation; and also in some cases of decided catarrhal inflammation of the alimentary mucous membrane, this kind of administration of morphia produces effects superior to anything which could be obtained before its introduction. Let it be clearly understood what is meant by purely stimulant doses of morphia. I mean such doses, whatever their exact amount (and this may range from $\frac{1}{12}$ to $\frac{1}{4}$ grain, in different subjects and varying circumstances), as produce relief of pain, natural sleep, and cessation of delirium, which check excessive epithelium growth on mucous membranes, and stimulate the repair of ulcerated parts, *without producing either stupor, contracted pupil, or subsequent constipation of bowels or burning of tongue.* Here is no vestige of *narcotic* action; and, after the disappearance of the immediate effect, there is no depression, no indescribable uneasiness, no yawning or sighing, no craving for the repetition of the dose. So long as such doses, only, are administered, and are administered only for the definite purposes above named, no opium-habit is set up: the patient can discontinue the injections the moment that the special need for them has ceased, without the least inconvenience. This constantly happens, for example, in the acute stage of pleurisy: again, in acute rheumatism I have never observed this kind of use of the syringe to prolong the state of enfeeblement and of liability to recurrence of pain in the manner which Dr. Allbutt describes. I think I shall be able to show, presently, how those unfortunate results are probably produced.

2. There is a second grade of hypodermic morphia action which we are all occasionally obliged to employ, but which is also very frequently induced quite unnecessarily, and with objectionable effects. The small purely stimulant dose of morphia will not always suffice to effect our object, That object may be, for example, to relieve, with all speed, a pain so violent as to be directly depressing in a mischievous or even dangerous degree. Even here it is our duty, in the first instance, to try the small dose, and it is surprising how often it will succeed: but in a certain number of cases it will quite fail; and we must then frankly acknowledge to ourselves that it will be necessary to *narcotise* the patient to a certain extent. We then administer such a dose (*e.g.* $\frac{1}{4}$ grain or more) as will quickly produce some stupor and contraction of the pupil. Now if the course of the illness be such that this process has to be at all frequently repeated, we soon discover that our weapon is two-edged: the morphia-habit, already described, becomes fully developed, and, whatever else may happen, we may be sure, at least, that we shall have considerable trouble afterwards in inducing the patient to give up the medication. But a more substantial evil is

sometimes produced. If the temporary mischief, to remove which we have consented to the induction of narcotism, be not conquered by a very few narcotic doses, there arises a new condition of things. Narcosis, it can never be too frequently repeated, is *a depression of nervous life*. At first this depression is so slight and temporary that (in vulgar parlance) it is spoken of as merely "functional," though, assuredly, alteration of function is never without its correlative in modification of tissue. From a few such temporary depressions of its life the nervous system has sufficient recuperative power to recover quickly and completely; but if the morbid process be repeated too frequently and too strongly, there is no such recovery, or only a partial one. The outcome of such a series of actions is, that the nervous system enters upon a new kind of physiological life, in which it is unable to work harmoniously without the constant presence, in the blood that feeds of it, of a calming and regulating agent like morphia.

3. This kind of evil reaches its highest development in those unhappy cases where, from one cause or another, an individual has long adopted the habit of using large doses of morphia every day or more frequently, and has now come to employ a very large daily allowance of the drug. Such a state of things may have arisen from mere wanton indulgence on the patient's part, or it may have been induced by the ever-present agony of an incurable malady (such as neuralgia of the worst type, or malignant disease), and the medical attendant may have been reluctantly compelled to acquiesce in a medication which he must nevertheless deplore. The injurious effects of this kind of habit are plainly perceptible in the general enfeeblement of nervous power of all kinds: the intellect and (even more strikingly) the moral energy steadily decline in power; but the most notable downward tendency is, after all, on the side of the sensory nervous system; for although temporary relief to pain can always be procured by a sufficiently large dose of injected morphia, the tendency to renewal of the pain becomes more and more pronounced, and a notable degree of hypersensitiveness to trivial sources of irritation becomes more and more apparent from day to day and from week to week. Moreover, if the injection be now omitted, the pain returns with an agony incomparably more severe than that which would attend the disease when uncomplicated by the results of the prolonged use of morphia. Besides the effects visibly and directly produced through the nervous system, there are changes of nutrition in the tissues; at least I am under a strong impression that a particular kind of muscular atrophy, which especially attacks the facial muscles, is one of the results of large and long-continued doses of hypodermic morphia. This effect, I believe, is also

observed in ordinary opium-eating, and gives rise to the curiously *lined* appearance of the face, which is different from the results of simple emaciation as is also another occasional symptom, the deposition of a considerable amount of brown pigment in the skin of the face.

The three degrees of action of hypodermic morphia above described certainly represent very different physiological facts, and they are no less different in their practical relations to treatment. Let me insist, first of all, on the narrow limits within which, alone, the physician ought to tolerate the third or highest degree of the medication. Considerable experience has convinced me that there are only two kinds, even of incurable pain, in which we really need yield to the tendency to increase the daily dose to a very high point: the pains, namely, that are produced by steadily progressive *ulcerative* processes involving nerves, or by continuous and increasing *pressure* on nerves from a tumour (cerebral tumour affording, perhaps, the most exquisite examples of this last.) I particularly wish to say that, in my belief, the necessity need never arise in chronic incurable *neuralgia* if the medication be properly managed from the first. Having been much engaged in the treatment of neuralgic affections during some years past, I have had occasion to hear the past history of several patients who, when they first came before me, had already arrived at the daily use of very large hypodermic doses of morphia; and, without exception, I have found that their medical attendants had commenced the hypodermic treatment by doses which were unnecessarily large for that stage, and which, in fact, from the first always produced considerable narcosis. Now, in treating my own cases, I have found that this practice is not merely superfluous, but that it ought to be avoided, as it is sure to produce needless mischief; and I shall here give a typical example of the way in which I have been gradually led to treat the class of cases now referred to.

The patient is a lady in advanced life, who some seven years ago was attacked, for the second time, with sciatica. The first attack had occurred some thirty years previously: the malady was then extremely severe and obstinate, but at length departed, leaving her free from anything like localised neuralgia until the commencement of the illness from which she still suffers, and the immediate exciting cause of which was anxiety of mind. This lady is herself of a highly nervous and emotional turn, and comes of a family in whom the emotional and æsthetic mental temperament has always been conspicuous. One fact in her case deserves particular record, as showing the extraordinary power which nervous *commotion* exerts over her organism. She has always had abundant hair: this was bright

brown in her youth, but comparatively early in middle life it began, under the influence of repeated anxieties, to turn grey. But the remarkable circumstance (which all her friends can verify) is that, without the falling out of a single hair, the whole mass would fluctuate backwards and forwards between brown-grey, iron-grey, and almost snow-white, in the course of a few weeks or even days, according as her mind was calm or disturbed. Even now, though the hue is far more permanently grey than it was a few years since, there are considerable fluctuations from time to time, according to the state of her nervous system. The sciatica was from the first very violent and intractable, and unfortunately the patient was unable or unwilling to take that prolonged and complete repose in the recumbent posture without which no sciatica, certainly no genuine and severe sciatica occurring in advanced life, can be cured. The consequence was that all hope of cure had at length to be abandoned, and mere palliation, by morphia, was the best course remaining open. For some time the patient took the drug by the mouth ($\frac{1}{4}$-grain doses), as I did not see my way to providing her (in a strictly country place) with the means of daily hypodermic injection. However, about three years since, I overcame these difficulties, by carefully instructing her servant in the use of the syringe. I commenced with either the $\frac{1}{11}$th or $\frac{1}{12}$th of a grain (I am not quite sure which, but remember distinctly the surprise of finding that so small a quantity produced twenty-four hours' complete immunity from the pain); and notwithstanding the daily use of the injection up to the present time, we have only increased the dose to $\frac{1}{4}$ grain, at which point it seems likely to stand for some time to come. It is difficult, without using language that would sound extravagant, to indicate the value which hypodermic morphia has had for this lady. The agonising sciatic pain was shattering her whole nervous system, and her general health was in grave danger of a fatal collapse, when we commenced it. Since she has used it, she enjoys a great deal of calm happiness, her mind is active and clear, though her emotions are still very easily excited; she has comparative, though not complete, immunity from pain, and her general nutrition is unquestionably in a very good state for her time of life.

It is very interesting to analyse and estimate, in this patient, the truly narcotic effects; for such they are, though they are at a minimum, in consequence of the strict economy with which the morphia has been used. There is *very slight* contraction of the pupil observable during two or three hours after the injection. There is occasionally a slight excess of sleepiness, making it rather difficult for the patient to rouse herself in the morning; and towards three o'clock in the afternoon (about two hours be-

fore the usual time for injection) the beneficial influence of the morphia has usually given way, not (save exceptionally) to positive *pain*, but to the indescribable sense of depression which Niemeyer's patients so graphically represented to him by the phrase "katzenjammer." This feeling disappears almost immediately after the injection, the patient enjoys her dinner, is lively and conversational during the evening, and goes to bed at 11, or later, to enjoy, most commonly, what would be considered a very good amount of sleep for a person of her age. In fact I see no reason to doubt that, if preserved from fatal acute disease, this patient has many years of a by no means unhappy or unfruitful life before her; certainly not free from suffering, but with no unendurable suffering. Whereas I cannot doubt that, but for the medication, she would now have been far advanced towards fatal exhaustion, if, indeed, that point had not been already reached.

There can surely be no doubt that such a method of employing the hypodermic injection as the above is perfectly legitimate. Granting fully that we have here a fully formed morphia-habit, difficult or impossible to abandon, it does not appear that this is any evil, under the circumstances. I would even venture to say that, for elderly persons afflicted with an incurable painful disease, such a result of treatment affords an ideal of what we could desire.—*Practitioner, March* 1870, *p.* 148.

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## 104.—BAIN'S AND PACINI'S METHODS OF RESTORING SUSPENDED ANIMATION.

Report of the COMMITTEE OF INVESTIGATION appointed by the Royal Medical and Chirurgical Society.

[The inquiry was pursued by means of experiments upon the dead human body. Four subjects were placed at the disposal of the Committee by the authorities of St. George's Hospital, by which means they were enabled to make eighty-three observations.]

*Method of investigation.*—In order to ascertain the relative merits of the two methods referred to the Committee, not only to each other, but also to other methods already in use, they were contrasted with the plan proposed by Dr. Silvester, the three modes being employed alternately on the same subject. In some of the experiments the operator was the same in all three classes of cases; in others, the Committee had the advantage of seeing Dr. Silvester and Dr. Bain perform their own experiments in the manner advocated by themselves.

The means which were employed to ascertain the actual quantity of air alternately introduced into the respiratory

cavity and expelled therefrom by the several mechanical expedients was the instrument designed by Dr. Sanderson, and employed by a former committee of the Royal Medical and Chirurgical Society appointed to report on the subject of suspended animation. In all the experiments the apparatus was directly connected with the trachea.

The mode in which artificial respiration is performed in the two methods under investigation is thus described by their respective advocates.

Professor Pacini describes his plan in a pamphlet, entitled "Di un nuovo Metodo di Practicare la Respirazione Artificiale," as follows:—"Supposing that the asphyxiated patient is on a horizontal plane above the ground, as a bed or a table, the operator stands with the head against his own abdomen, and then with his hands takes a firm hold of the upper part of the arms, applying the four fingers behind and close to the armpit, while the thumb is in front of the head of the humerus. Having thus seized both shoulders, he then pulls them towards him, and then lifts them in a perpendicular direction, by which means the sternum is first raised by means of the clavicle and, in consequence, the ribs, which, diminishing their obliquity with the spine, enlarge the thoracic cavity both in its transverse and antero-posterior diameters. If the patient is lying on the ground, it is easy to understand that the operator may then place himself on his knees so that the head may remain firmly placed against them. It is, however, necessary to explain that, in order to effect more fully the elevation of the chest, the rest of the body should not be allowed to yield to the traction, but should be fixed; so that, when the weight of the body does not offer sufficient resistance, an assistant should use counter-extension by holding the feet or by fastening them to some immovable object."

Dr. Bain thus describes his method:

"The patient being laid on his back on a table, if convenient, the mouth and nostrils are to be wiped dry, the clothes from the upper part of the body, at least, having been removed. The operator stands at the head of the patient, placing the fingers of each hand in the axilla in their front aspect, with the thumbs on the clavicles, and pulls the shoulders horizontally towards him with a certain degree of power. Upon relaxing his pull the shoulders and chest return to their original state."

Dr. Bain also occasionally employs another plan, which he terms his "second method," and which he thus describes:

"The shoulders are elevated by taking hold of the hands and

raising the body about a foot off the table, the arms being elevated at an angle of 45° over the head."

The following are the results of the observations:

*June 23rd*, 1869.—*At St. George's Hospital.—Present.*—Dr. Sanderson, Dr. Bain, Mr. Savory, Mr. Power, Mr. Gascoyen, and Mr. Pick.

SUBJECT I.—A well-formed woman, aged 56, who had died of obstruction of the bowels, and who had no disease of the lungs. Had been dead twenty-one and a half hours.

*Observation* 1.—Dr. Silvester's method. Body lying flat on the back, with the head resting on the table. Elevation of both arms was attended by the introduction of 18 cubic inches into the lungs. On replacing the arms to the side 18 cubic inches of air were expelled from the lungs.

*Obs.* 2.—Repetition of the above. On elevation of arms 20 cubic inches were inspired. On restoration of arms to side 22 cubic inches were expired.

*Obs.* 3.—Repetition of the above. On elevation of the arms 20 cubic inches inspired. On restoration of arms to side 20 cubic inches expired.

*Obs.* 4.—Dr. Bain's method. The body lying flat upon the back. The shoulders were firmly grasped and drawn upwards in the direction of the ears. This was attended by the introduction of 26 cubic inches of air. Upon allowing the body to return to its natural condition 26 cubic inches were expired.

*Obs.* 5.—Repetition of Experiment 4. On elevation of the shoulders 24 cubic inches were inspired. On return of the body to its natural condition 24 cubic inches were expired.

*Obs.* 6.—Repetition of Obs. 4 and 5. During the inspiratory process 26 cubic inches were introduced into the lungs. During the expiratory, 26 cubic inches were expelled.

*Obs.* 7.—Dr. Silvester's method. Repetition of Obs. 1, 2, 3. The elevation of the arms was attended with the introduction of 21 cubic inches into the lungs. Upon replacing the arms to the side 21 cubic inches were expelled.

*Obs.* 8.—Upon repeating this experiment 22 cubic inches were introduced, and 22 cubic inches expelled.

*Obs.* 9.—A third repetition of the experiment was attended by the introduction of 23 cubic inches, and the same amount was expelled.

*Obs.* 10.—Dr. Bain's method. Repetition of Experiments 4, 5, 6. During the inspiratory process 29 cubic inches were taken into the lungs, and 28 expelled from them during expiration.

*Obs.* 11.—Upon repeating this experiment 25 cubic inches were introduced and 24 expelled.

*Obs.* 12.—A repetition of Obs. 10 and 11 was attended with

the introduction of 26 cubic inches and the expiration of 25 cubic inches.

*Obs.* 13.—Dr. Silvester's plan. The shoulders of the subject were now raised by placing a block under the chest and the head thrown back. Elevation of both arms in the same manner as in the first experiment was attended by the introduction of 27 cubic inches of air into the lungs. On replacing the arms to the side 24 cubic inches were expired.

*Obs.* 14.—Repetition of Obs. 13 was followed by the introduction of 24 cubic inches and by the expiration of 23.

*Obs.* 15.—This experiment repeated produced 23 cubic inches during inspiration and 24 cubic inches during expiration.

*Obs.* 16.—Dr. Bain's method. The subject being placed in the same position as in the last three experiments, with the shoulders raised. During the inspiratory process 25 cubic inches were introduced; during the expiratory 24 cubic inches were expelled.

*Obs.* 17.—Repetition of the last observation. The inspiratory process was attended with the introduction of 24 cubic inches, and the same amount was expelled during the expiratory process.

*Obs.* 18.—On the third experiment the amount introduced was 25 cubic inches; the amount expired was 24 cubic inches.

*Obs.* 19.—The last three experiments were repeated, Mr. Power (instead of Dr. Bain) performing them. Inspiration was attended with the introduction of 27 cubic inches. During expiration 26 cubic inches were expelled.

*Obs.* 20.—Upon repeating Obs. 19, 26 cubic inches were inspired, 25 were expelled.

*Obs.* 21.—A repetition of this produced 27 cubic inches inspired, 26 expelled.

*Obs.* 22.—Pacini's method. The method recommended by S. Pacini was now tried. In the first experiment (performed by Dr. Bain), during the inspiratory act 16 cubic inches were introduced into the lungs; during the expiratory act 16 cubic inches expired.

*Obs.* 23.—Upon a repetition of this experiment 20 inches were inspired and 20 expired.

*Obs.* 24.—This repeated produced a result of 21 cubic inches introduced during inspiration and a like amount expelled during expiration.

*Obs.* 25.—These last three experiments were repeated, another gentleman (Mr. Power) performing them. During the inspiratory act 34 cubic inches were introduced; during the expiratory 30 cubic inches were expelled.

*Obs.* 26.—This repeated produced during inspiration 32 cubic inches introduced; during expiration 32 cubic inches expelled.

*Obs.* 27.—On a third experiment, the introduction of air was 32 cubic inches; the amount expelled was the same.

*Obs.* 28.—Dr. Bain's second method. One experiment was performed. This produced during inspiration the introduction of 31 cubic inches of air; during expiration 30 cubic inches were expelled..

[Several other experiments were tried, and in various ways. The paper ends with the following practical *conclusions.*]

From these experiments it appears that more air is introduced as a rule by traction from the shoulders than from the forearms and arms. Nevertheless it will be seen that in the amount of air introduced there is a greater difference when the same method is adopted with different bodies than there is between the two plans when practised upon the same body—this great difference being chiefly due to the size of the body, especially the amount of the mobility of the walls of the chest, and the rigidity of the muscles.

By either plan the Committee are of opinion that a sufficiently large quantity of air is without difficulty introduced. And it may be observed that in either case on an average more air is changed than in the act of ordinary tranquil respiration.

In estimating the relative merits of the two plans they are anxious to observe that other considerations are involved than that of the absolute and comparative quantity of air changed.

They are unanimously of the opinion that the method advocated and practised by Dr. Bain is but a modification of the plan usually known as Silvester's, and involves no new principle of action. Indeed, in his more recent publications, Dr. Silvester has not limited his point of traction to any one part in particular of the forearm or arm. They are, therefore, of opinion that in the great majority of cases it is of comparatively little moment which method of manipulation is practised, provided the common principle on which both are founded be fairly carried out.—*Medico-Chirurgical Transactions*, 1870, *p.* 291.

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## 105.—ON DEATH BY DROWNING AND COLD.

By Dr. BENJAMIN W. RICHARDSON, F.R.S.

I have taken advantage of the late extremely cold weather to institute a new series of researches on death by drowning in water at 32° F. This is the season in which such inquiries can only be satisfactorily carried on in our country, and it is the season, also, in which accidents occur that call forth our special curative skill. The study, consequently, is both practical and

philosophical, and I do not know in which of these directions it is most absorbing. To be able, by any line of study, to see the way more clearly towards lifting up those who have suddenly fallen into death, and who, but a moment before, were in the full of life, is a good effort, even in imperfection of grand result; and if attended in the end with affirmative result, would be, of all efforts in the practice of our art, the grandest; so that, in practice, the enterprise towards any improvement of treatment is worthy any amount of labour. Philosophically, the study is equally worthy, for it leads us to contemplate the physical phenomena of death with an appreciation which nothing less than an experimental research, tending towards the highest of intellectual advancements and the solution of the most solemn of mysteries, can develope and sustain.

I wish us, if you please, to keep these two lines of thought in our minds as we proceed; to think first of the practical, as experiment teaches us fact; to think of what is suggested for practice by each experiment; but to think, also, of the general teaching that may be gleaned, of the alliances of conditions of disease and of death, of the reasons why phenomena of death are manifested, and of the methods by which the phenomena may possibly be averted or removed in various forms of disease.

*Two Forms of Real or Apparent Death.*—When an animal body ceases to render to any of our senses evidence of motion, voluntary or involuntary, we are accustomed in the common range of our knowledge to think it is dead. If, when we try to make the body move of itself, we fail in the effort, we usually affirm the fact of death; but if there is to be detected the merest ripple of motion in any part or organ, we hesitate to pronounce the word. In a certain rude sense we are correct in this mode of reasoning, for death truly is inertia of all that was actual or capable of motion under the *régime* of life. But in a scientifically strict sense we are not correct, for the inert body may not be dead; it may be simply in a condition of apparent death, from which it might recover, or it may be in a condition from which, according to our present light of knowledge, it is absolutely irrecoverable.

Thus I would begin by pointing out this truth; and, as far as I know, it is the first time the same truth has been clearly defined: that when motion ceases in an animal, the body settles into one of two physical states:—(*a*) a state in which the water and crystalloidal matter are fixed, the fatty matter solid, but the colloidal matter still hydrous and ready for action; (*b*) a state in which the water and crystalloidal matter are free, but in which the active colloidal matter has become pectous—has passed, that is to say, from the hydrous to the pectous condition. I propose, therefore, to divide the process of death imme-

diately sequential to the arrest of motion into these two conditions—

| 1. | 2. |
|---|---|
| Crystalloidal. | Colloidal. |
| Glacial. | Pectous. |
| Solutive. | Insolutive. |

As a rule, the immediate condition of the body after arrest of motion of life is the second condition named above, the pectous or insolutive; for so soon as the animal motion has ceased, the tendency of the fibrine is to become insoluble, and to settle into the solid or pectous form. Only one circumstance connected with death can, as far as I know, prevent this change, and that is, exposure to great cold. If the body, at the time of death, is so placed that it is quickly reduced below 45° Fahr. in all parts, the pectous change will be avoided, and the solutive or glacial condition will be established. As I shall illustrate presently, we can, by experiment on certain animals, induce the pure glacial death; and there are some facts which tend to prove that the same kind of death may happen even to man. But let us at once understand that the event in the human subject is extremely rare.

It is essential to have the two conditions immediately following death, of which I have spoken, in the clearest appreciation; and especially so when we turn from the study of condition to the study of treatment. For here at once is a fact of primary interest: the state which I have called glacial or solutive is one from which we may secure recovery; the pectous or insolutive is one from which there is, according to our present light, no means of recovery. Moreover, as the glacial lapses into the pectous condition very easily on mere elevation of temperature, it is worth remembering that in processes of recovery we may, if we are not careful, transform the recoverable into the irrecoverable death.

*Characteristic Features of the Glacial and Pectous Conditions.*—Let us now turn to the physical characters of the animal matter in and during the two conditions named. At first the animal tissues and fluids are closely alike: there is appearance of solidity, with continuation of form. Here, for example, are two eggs from which the shells have been removed; both are solid, both admit of being cut by the knife, and both, when cut, present the same general appearances of surface. But these eggs are in entirely different conditions: the one is in the state of solidification from cold, which we call "glacial;" the other is in the state of solidification from heat, which I call "pectous." The first is re-soluble without chemical decomposition of its parts; the second is insoluble unless it be allowed to decompose. Here are two specimens of blood, both solid, or rather semi-

solid, looking each as if clotted; they were taken from the same animal—a sheep—two hours ago, and in colour, consistency, general character, they are still as one; but they are quite different in this physical respect—that one is in the glacial, the other in the pectous condition of solidification. I take the first, gently warm it, and it liquefies; I take the second, gently warm it, and it remains solid; in short, while the first specimen could be brought back to the condition of natural fluid blood, the second is immovable. It is worth while before we move from these specimens of blood to make a further observation. In these bloods the fibrine is the colloidal substance that undergoes change. Now, fibrine at rest and out of the body passes into the pectous state whenever the fluid of which it forms a part is at or above a temperature of 45° Fahr. If, consequently, in the process of thawing blood that has been made to undergo glacial solidification, we raise the temperature too suddenly, we may transform the glacial condition into the pectous without witnessing the intermediate stage of fluidity, so rapidly will that pass over. In order, therefore, successfully to thaw frozen blood, we must put the glacial mass into a metal cup that is immersed in water above freezing-point, but not above 45° Fahr., and we must add a little more frozen blood if solution is taking place too quickly. In this manner we can get the blood fluid, and can hold it fluid for a long time by simple management of temperature. I press this point forcibly on the attention, because it explains the cause of many dangers in efforts for recovery, local or general, after glacial solidification.

We pass from the specimens of blood to something more telling. Here are two animals—two carp; both are what would be called dead; both are stiff, motionless, cold. Without experiment, I could not myself tell the condition of one from the other, were they brought before me accidentaily; yet they are so far different that one of them is actually *not* dead, while the other is irrecoverably dead—the one is locked up motionless in glacial, the other in pectous death. I take the fish that is locked up in pectous death, and manipulate it as I will, but I cannot make a muscle move; I take the fish in glacial death, and, if I bring sufficient care to bear, I can unlock the fixed muscles, restore circulation, and restore life. I will try this experiment. I place the fish solidified by cold in a globe containing ice-cold water, with a little ice floating in the water, and then I pour in hot water, taking care, if I can, that the ice is not melted too quickly; and, if my arrangements are correct, the fish in a minute or two will move briskly. It does not move, and I see I have failed. I have failed for want of due care; I have raised the temperature of the water too high, and have transformed the glacial into the pectous form of death.

We need not regret this experimental failure, for it is a useful lesson, indicating how easy is the transition from one state to another. We will simply take a second carp, that has been also locked up motionless in glacial death, and try again, using more care to restore. This time we succeed; the rigid animal rapidly relaxes, and leaps even out of the water as it is resolved into life. For a time it will remain dull, but it will recover as perfectly as though nothing had happened to it.

This experiment, on one single fish, is but representative of a larger experiment sometimes seen. Not long since, Mr. King, the well-known naturalist, of Portland-road, had sent him from Newcastle a cargo of gold fish. The fish had been placed in tepid water; but, in journeying to London, water and fish too were fixed by intense cold, and when they arrived at their destination, the animals were all intensely rigid, and apparently dead. On pouring heated water upon the ice, and so dissolving it gradually, the fish recommenced to move in the water, and, as I learn from Mr. King, who himself carried out the proceeding, they all recovered. The experiments thus described can be performed on frogs in an equally determinate manner. When, however, we ascend to the higher class of animals, the process of resuscitation from glacial death is more difficult; the sources of difficulty being that the whole mass of the animal body cannot be equally and simultaneously resolved, and that in the higher animals the blood passes much more quickly into the pectous state; but what we have seen illustrates with sufficient clearness the distinction, in respect to the restoration of life, between glacial death and pectous death.

I have already said that the two conditions resemble each other closely. It is so. In both states the muscles may seem firm and rigid; in both the blood may seem solidified if a vein be laid open; in both the crystalline lens may appear opaque; and in both all motor phenomena of life may be entirely absent. But here is the distinctive difference: In the glacial death the active colloidal structures remain hydrous and ready to react; in the pectous death they have taken on a new state—they have been rendered molecularly inactive in respect to motion of fluidity, and cannot be restored to their original condition.

I have said that the outward signs of death are much alike in both kinds of death. I should add now that the more refined evidences of death answer equally in both cases. In glacial death, while it lasts, the muscles fail to respond, unless heat be introduced, either to mechanical or electrical excitation. When we lift up a part of the body that is thin and semi-transparent there is no colouration from vascular current; nay, when we try Laborde's ingenious experiment of thrusting a needle into the muscles of the animal the steel comes out unoxidised. In a

sentence: In the pure and extreme glacial death there is perfect cessation of motion, as far as we can gauge, and yet the functions are not impossibly prevented, for we know how to restore some animals by the simple process of restoring heat in a gradual and scientific way.

And still, with all the similarities, there is the marked difference that the glacial condition can, for a given time, be recovered from—and perhaps for any time can be recovered from—if evaporation be prevented; while the pectous state cannot be resolved by any known method. If the resolution of the pectous condition back to the fluid condition could be secured without change of molecular constitution, we should indeed make progress; but up to the present time we have not succeeded in the attempt. Here is this pectous or coagulated egg; here is this pectous or coagulated blood; here is this pectous or coagulated carp—the problem is, how to bring them back from the insoluble to the soluble state without changing their character and their properties; how to bring them back in the same way as we bring back the glacial egg, the glacial blood, the glacial animal. Whoever may effect this transformation is a master in science.

Some years ago I thought, in respect to blood, I had, in an imperfect manner, resolved the difficulty. I took coagulated blood, triturated the mass with ammonia until it became fluid, and then extracted the volatile alkaline solvent by exhaustion under the air-pump; and it is true that a process like the process of recoagulation could afterwards be induced. I fear I was deceived, and that the condition of solidity induced was not the true pectous condition; it was like it, but was not the same.

I must leave these definitions of the two states of animal bodies after death, in order to pursue the rarer of the two states—that in which the glacial condition remains for a longer or a shorter space of time. We have seen that in fish and in frogs the body may be brought to such a degree of cold that the process of pectous change may be suspended. It remains to be asked whether the same may occur in the higher animals, and in the highest animal in the scale of created beings on this planet—man.

Touching warm-blooded animals, I may say that, after many experiments, I have once known a recovery of a kitten after no less than two hours' immersion in ice-cold water; but I have repeated the experiment several times without obtaining the same result. Once, also, the following experimental fact occurred to me, and it is of singular interest:—A dog was put to sleep with the vapour of chloroform during a time of intense frost. The animal died in the vapour, and the body was opened.

after death, and was left exposed to the open air with an injecting tube tied in the ascending aorta. Three hours after exposure to the cold air, the thermometer registering six degrees of frost, water at 130° was injected by the tube through the arteries of the animal. The result was most singular: as the warm fluid traversed the body, every muscle appeared to be brought into action. The intestines first showed active vermicular action; the muscles of respiration followed; and, finally, the muscles of the body generally, commencing with the facial, took on movement. At one moment it seemed as if the animal were alive again; but as the injection was continued the muscles settled into persistent contraction—they had passed through an interval of motion, during which they had been transformed from the glacial into the pectous state.

*Observations on the Human Subject.*—Observations on the human subject after death by simple cold have been very imperfect, but facts have been elicited which demonstrate that even in man the pectous change, in the muscles at least, may be suspended for long intervals of time, and that the muscular motion may be reinduced under the stimulus of heat. The great experiment of Aldini—in which a man who had been executed was made to exhibit such extreme excitability of muscle that he seemed to live again—was of this character, the man having been submitted to the executioner on a day when the air was extremely cold; and later experiments have confirmed what was seen by Aldini and his fellow-observers. In cases of death by drowning in water at freezing-point, some truths even more practical have been brought under notice. Last year, my friend, Dr. Belgrave, of Hendon, was summoned to attend two men who had been immersed in the lake at the Welsh Harp, Hendon. One of these men was extricated from the water by Dr. Belgrave himself after twelve minutes of immersion, and, artificial respiration having been immediately set up by Silvester's method, the man recommenced to breathe naturally, and lived for several hours—the death taking place, ultimately, from what Dr. Belgrave considers to have been congestive bronchitis. It was unfortunate that this patient was conveyed quickly to the house, and, before the Doctor could control the treatment (the second man being also under his supervision) was placed in a hot-water bath, a method which, as I shall show in my next lecture, is opposed in every particular to recovery.

In this case, then, there was an example of a man who was subjected to drowning and cold at freezing-point for twelve minutes, and in whom, nevertheless, the muscles remained capable of excitation and the brain of consciousness (for the man became conscious), and who, on restoration of respiration, lived again.

A case such as that described by Dr. Belgrave, and so thoroughly supported, leads to the conclusion that, in sudden death by drowning and cold, the body of a warm-blooded animal may be so left that the colloidal part shall not for a long period become pectous, but that it shall be left in a condition for recovery even after a prolonged immersion. I have been led thereupon to endeavour to ascertain, by direct observation on the inferior warm-blooded animals—first, what are the phenomena of death after the same mode of death; and, secondly, what are the obstacles to recovery. The first of these inquiries I will proceed to explain, in so far as I have obtained information; the second I will reserve for another of our meetings.

*Mode of Death and Condition of the Body of Warm-blooded Animals, after Drowning and Cold.*—When strong and healthy warm-blooded animals fall in water at freezing-point, the death from the drowning, if death follow, is, as a rule, remarkably rapid, and apparently free of pain. The same obtains in the human subject; for, in the case of a woman who was immersed in water at freezing-point, who was taken out in an unconscious state, and who remained unconscious for more than twelve hours, I had the opportunity of putting to her the question of her sensations at the time of the accident. She affirmed to me that she felt nothing but an indescribable intenseness of cold, which seemed to make her body feel smaller, followed as quickly by an utter and helpless exhaustion, after which she remembered nothing. It was proved by those who were engaged in dragging her out of the water, that she struggled severely, and was convulsed; but of this she had no knowledge whatever. In strong animals we sometimes see convulsive movements when the body is under the water, and these may occur a few seconds after what appears to be the quiescence of death. The whole of the phenomena, up to the time of absolute cessation of motion and of consciousness, are included in the period of a minute, probably, in every case, and I have seen them included in thirty-five seconds. In very young animals the struggle for life is longer than in old, and in animals well primed with food, and well fed, the struggle is longer than in animals requiring food or imperfectly nourished; but in all it is very short I refer to these facts, because to those who have been so unhappy as to lose relatives or friends by drowning and cold, it is a satisfaction, sad as it may be, to know that the death is free from prolonged suspense and from acute pain. Indeed, I should judge it to be one of the easiest of deaths—as easy as death by chloroform. The convulsive movements that are seen are unconscious movements; they are the same as those which mark the period of stupor, in death by hanging, by narcotic vapours, by concus-

sion; and they are simply the results of action of muscles from which the controlling power of the nervous centres has been removed.

Various speculations have been offered respecting the suddenness with which those who are immersed in water at freezing-point sink in it so readily, the favourite being that the sufferers are subjected to what is commonly called cramp of the extremities, and are thus prevented from exerting themselves, even if they be swimmers, so as to escape from death. My own view differs from this. It is clear, I think, to all who have witnessed the phenomena of death by drowning and cold, that the cause of the sudden and complete collapse is peripheral nervous shock and sudden exhaustion of the nervous centres by direct extraction of animal heat. It would be most correct to say that the death is by nervous syncope; for although there is no actual loss of blood from the vessels, there is loss of the force the blood supplies, which amounts practically to the same thing.

If a body be removed from the water in from one to two minutes after complete immersion, the muscles everywhere are found perfectly flaccid; and assuming the convulsion which always precedes death shall have been passed through, the muscles will be found motionless as well as relaxed. The voluntary muscles will be found at first very feebly excitable; the respiratory muscles more excitable; the heart not simply excitable under stimuli, but acting of itself, often in perfect rhythm and in all its parts. Thus, the heart in these cases, as in cases of hanging, poisoning by narcotic vapours, and hemorrhage, continues to the last true to its duty. The action of the heart is, however, very feeble, and it is not sustained long as an independent motion. I should limit the duration of action to five or six minutes.

The heart and all the other muscles, motionless and flaccid though they are, are nevertheless capable of showing vigorous action when they are supplied with heat; and under the influence of heat they soon undergo true rigor mortis. If a dead animal just removed from water at freezing-point be laid on one of its sides upon warm sand, sand at 115° Fahr., the whole of the muscles on that side will pass into firm, pectous rigidity, while the muscles on the side exposed to the air will remain flaccid. We may thus witness the curious phenomenon of intense rigor mortis and flaccidity in the same animal at the same time.

The condition of the internal organs of the body immediately after death by drowning in water at freezing-point is favourable to recovery, as you will see here in the body of an animal—a rabbit—that was removed from the water five minutes after the cessation of respiration. The conditions are fairly representative of what is always seen after this form of death. The

brain and spinal cord are free of congestion, the cut surfaces free of vascular line or speck, the structure rather firm; the sinuses hold blood, but are not distended. The heart is charged with blood on the right side, but not to distension; the pulmonary artery contains blood in its trunk and branches; the left side of the heart is contracted, but contains a little blood both in the auricle and the ventricle; the arteries are empty, indicating that the final arrest of the circulation of the blood was in the pulmonary tract of blood. The abdominal organs are natural, neither congested with blood nor pale. The muscles are still flaccid, and, under the influence of heat, are capable of undergoing contraction.

We should infer, from what we see here, that for some time after death, a body so little injured and so ready for motion ought easily to be set again in motion. It does truly often occur to my mind as if the merest device would be sufficient to bring back the phenomena of life. What is more, if the body already drowned be left in the water at freezing-point, it will retain the favourable conditions I have described, not for minutes merely, but for hours—as long, probably, as the carp which we brought back to life at the early part of the lecture. I am certain, too, that the day must come when this suggested restoration of the higher animal will be matter of fact, miracle as it now may seem.

Against such success at this moment there are certain obstacles which observation has rendered clear to view, and there may be obstacles not yet foreseen; but those known are sufficient to engage our attention, and they are fatal unless they can be removed or overcome.—*Medical Times and Gazette, Feb.* 18, 1871, *p.* 181.

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## 106.—POISONING BY STRYCHNIA SUCCESSFULLY TREATED BY BROMIDE OF POTASSIUM, WITH SOME REMARKS ON THE THERAPEUTICAL PROPERTIES OF THE BROMIDES.

By Dr. CHARLES B. GILLESPIE, of Freeport, Pa., U.S.

[The patient had taken three grains of strychnia two hours before being seen by Dr. Gillespie.]

His pulse was 70, hard and contracted; respiration good. The whole surface of the body was quite cold; great anxiety in the expression of the face; sight and hearing perfectly normal. On giving him drinks, the great difficulty was in getting the cup to his mouth without throwing him into convulsions; but when once there, he would gulp the contents down spasmodically in great mouthfuls. He had but little control over his arms; as soon as he let go his grasp on the bedstead they would jerk

violently, and continue thus until he laid hold of something solid and immovable.

The spasms were evidently becoming more violent and frequent, and beginning to implicate the muscles of respiration. Not having the remedy I desired with me, I gave him a teaspoonful of the fluid extract of hyoscyamus, and then hurrying home, weighed out one ounce of bromide of potassium, which I dissolved in three ounces of water. Of this solution I ordered one-half ounce every thirty minutes; and I felt so confident of its efficacy in this case that I intrusted the administration of the remedy to a carefully instructed attendant, and did not revisit the patient till next morning, when I found him out of danger. The paroxysms had gradually become less violent and frequent, and by the time the last dose of bromide was taken at midnight, he was able to get up without assistance and walk to his own room. The only bad effects remaining were excessive muscular and nervous prostration, with an occasional slight convulsive shudder, which, however, entirely passed off through the day, and in thirty-six hours' time he was up and at his usual business..

I am confident, that but for the prompt administration of large doses of the bromide my patient would not have survived. How much the large dose of hyoscyamus had to do with the result I am not prepared to say; it produced a free and painless catharsis, and may have aided in relaxing the spasms. But to the well-known effect of the salt over the anterior or motor portion of the spinal cord, the good result in this case is especially to be attributed. The physiological effects of the strychnia and bromide of potassium are, I am sure, directly antagonistic. In this case, the patient had taken fully two grains and a half of pure strychnia, for I obtained the paper in which the strychnia was wrapped, and found that all of it was taken save about one-half grain that adhered to the paper, and which I afterwards weighed. This certainly was sufficient to destroy life in any human being.

I may here be pardoned for saying something about my use of the bromides in the practice of my profession. The first manufacturers of bromine and its compounds, in this country, were Drs. David Alter and E. Gillespie, of Freeport, Armstrong Co., Pa. This was more than twenty-five years ago; and it is to them I owe my first knowledge of the bromides and their use in medicine. For more than eighteen years I have used the bromide of iron and the bromide of potassium almost constantly in my practice. The bromide of iron especially is a great favourite, using it almost entirely to the exclusion of iodine in its outward application as a resolvent. Internally, it has proven, in my hands, to be one of the best chalybeates in use. In chlo-

rosis, and all female irregularities, there can be nothing better. And in chronic diarrhœa, in urethral or vaginal discharges, it is a most valuable remedy. In many years of practice, I have never yet known it to fail in curing erysipelas, whether traumatic or iodiopathic. My method of treating erysipelas is to freely paint the affected surface with the solution two or three times a day, at the same time giving the remedy internally. In diphtheria and croup, it has been my chief dependence. In the anginose and malignant forms of scarlatina I have tried it effectually, but without much apparent benefit. In senile catarrh and chronic bronchitis, where there is much expectoration and cough dependent upon its exudation into the air passages, it is a first class remedy, taken into the stomach and used occasionally by inhalation.

My experience with the bromide of iron proves its great value. It is always reliable and safe, and in the form in which I use it, is quite palatable and easy of assimilation. My formula for preparing the bromide is the following:—

Take of bromine 1 pound, of water 4 pints, small iron nails 3 ounces and 2 drachms. Pour the water into a one gallon glass bottle, and then add the bromine, and lastly the nails. Keep the bottle in a moderately warm place until reaction ceases; the union of the bromine with the iron will develop a great amount of heat, for which reason the glass ought to be well annealed. After the heat has passed off, decant the solution from whatever impurities may be in the iron, and keep in well-stoppered bottles. The dose of this solution is ten drops or less, three times a day, and may be given in sweetened water, or, what is better, in simple syrup. For external use, the solution should be painted on with a feather or small brush. For children, or as an application to parts of the body, where the skin is thin and sensitive, the solution should be diluted with water. Sometimes, when applied undiluted to the thighs or inner arm, it causes a great deal of pain, which, however, may be easily removed by the free application of the spirits of turpentine.

I have had many years' experience with the bromide of potassium, and have used it in a variety of diseases. In rheumatism, it has at times been of signal service; in various forms of spasmodic complaints I had found it extremely beneficial, without, however, being at the time aware of its true physiological power over the spinal cord.—*American Journal of Medical Sciences, Oct.* 2, 1870, *p.* 420.

## 107.—MODE OF ADMINISTERING CHLOROFORM.

[The following is the mode of administering chloroform at St. Bartholomew's Hospital, by Mr. Bloxam, Chloroformist to the Hospital.]

The bottle is graduated so as to show the number of minims withdrawn, whilst the stopper is perforated and drawn out to a fine point. A few shakes of the bottle, pour out a little of the liquid on to a bit of lint laid lightly over the patient's face, and every now and again the lint is reversed as more chloroform is poured out. Mr. Bloxam introduced this mode of administration, and has been well satisfied with its results in some 2000 cases in which he has already employed it. Where a Clover's apparatus cannot be had, this method of Mr. Bloxam's seems likely to prove a very convenient substitute.—*Med. Times and Gazette, Feb.* 4, 1871, *p.* 126.

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## 108.—OIL OF PEPPERMINT AS A LOCAL ANÆSTHETIC.

By Dr. ALFRED WRIGHT, Finchley.

A few years ago, when in China, I became acquainted with the fact of the natives, when suffering with facial neuralgia, using oil of peppermint, which they lightly apply to the seat of pain with a camel-hair pencil. Since then, in my own practice, I in the same way frequently employ oil of peppermint as a local anæsthetic, not only in neuralgia, but also in gout, with remarkably good results; indeed, the relief from pain I have found to be almost instantaneous."—*Lancet, Nov.* 19, 1870, *p.* 726.

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## 109.—CLINICAL RECORDS OF ANTISEPTIC DRESSING BY CARBOLIC ACID.

A considerable interval has elapsed since we last made any special record of the treatment of wounds by carbolic acid in the London hospitals. At that time this agent was very generally applied in some form or other, and under various conditions; and the practice of those hospitals in which the experiment was made with earnestness sufficient to afford reliable data appeared to encourage a persistence in the careful and studious application of the system. It appears, however, that in many of the hospitals this subject has not survived that first glow of interest which a new thing rarely fails to kindle; and it would be interesting to know how far this result has been due to the fact of experimenters not perceiving that the application of the antiseptic system, as Prof. Lister applies it, requires not only the mastery of a theory, but the practice of an art and an exercise of judgment which it is not possible to acquire casually in the course of one or two trials.

In endeavouring once more to direct attention to this system of dressing, it is not our object to pronounce a judgment on its merits, but, believing that carbolic acid is a powerful agent

both for good and evil (as the following testimony sufficiently proves), rather to elicit what are the conditions which determine success in some cases, and failure or the production of positive harm in others. If we succeed in doing this, we think we shall contribute to an advance in the general knowledge of the subject.

ST. GEORGE'S HOSPITAL.—At this hospital, Mr. Holmes, after carefully observing its application by Prof. Lister, has given the carbolic antiseptic treatment a long and careful trial, which has been followed by such marked results as to raise it high in the esteem and confidence of the hospital staff. It has been found that the proportion of wounds that heal by first intention is decidedly greater than where no antiseptic is employed; in others, in which it has occurred, suppuration has been much less than might have been expected. It is a valuable safeguard against putrefaction, and it confers a remarkable immunity from surgical fever, and the other deviations from health which are apt to complicate the healing of wounds. Mr. Holmes's most marked successes have been in the treatment of abscesses, and in the conversion of compound into simple fractures.

MIDDLESEX HOSPITAL.—Mr. Hulke has applied the carbolic treatment to a variety of cases, with very good results; but he is not prepared to assert that it has any advantage over chloride of zinc, either as to the production of immediate union or in the maintenance of a healthy condition of discharges. He believes carbolic acid lotion (strength of two grains to the ounce) to be a very serviceable injection in chronic cystitis; the acid is not, he says, absorbed from the surface of the bladder, and in no case where it has been thus applied has he observed the train of symptoms and condition of urine which are said to be associated with its use.

LONDON HOSPITAL.—Mr. Couper has been kind enough to furnish us with the main results of the extensive trial which carbolic-acid dressing has received at his hands. Since September, 1869, he has used Lister's antiseptic dressing in about fifty important cases. The majority of them were conspicuous for the small amount of constitutional disturbance produced by very severe injuries. The patients after a few days were free from pain, they ate and slept well, and had an appearance of health by no means usual after extensive wounds. Measured by the thermometer, the wound-fever was of short duration, and was occasionally absent altogether.

In some instances, the urine was black for one or more days after the first dressing, but in no case was the carbolic acid absorbed into the blood in such quantity as to cause symptoms of poisoning. Mr Couper does not now regard the discoloration of the urine as of any moment.

The immunity from erysipelas and pyæmia was most marked. There did not occur one instance of pyæmia. One case only was attacked with erysipelas—a case of scalp wound, in which there was present an abrasion of the ear, which was not protected by any dressing; the swelling and tenderness distinctly spread from the face and neck to the scalp.

One patient took erysipelas nine days after the antiseptic dressing had been abandoned. He had recovered from an extensive compound fracture of the skull, in which the trephine had been used. Only one patch of skin granulations, less than half an inch broad, remained unhealed, when the carbolic dressing was laid aside in the belief that it had become too stimulant and prevented the granulations from drying up.

To apply the dressing successfully, it is, Mr. Couper believes, necessary to remember that carbolic acid is a stimulant as well as an antiseptic. If kept sufficiently long in contact with an *open* wound, so far from preventing the formation of pus, it stimulates granulations and increases and prolongs suppuration.

The aim should be to secure the antiseptic action without any stimulation of the wound. Generally this is best attained by stitching the skin edges accurately together with fine carbolised silk. By this means the skin itself is made to shield the deeper portions of the wound from stimulation.

The experiment of dressing two similar wounds of the calf—for which no stitches were used,—the one with strips of wet lint, and the other with Lister's dressing, showed the process of filling up by granulation to be materially slower under carbolic dressing thus used (or misused) than under wet lint.

Mr. Couper's experience further tends to show that an antiseptic state of the fluids within a wound is but one of many conditions necessary for union without pus. Complete apposition of the surfaces, and the absence of all gliding of one surface on the other, are at least as essential. Unless these conditions be secured the antiseptic is powerless to prevent suppuration. For this reason deep sutures often contribute to complete success. Carbolised catgut is the most suitable material for this purpose.

Seven out of eight successive cases of compound fractures of long bones were converted into simple fractures, either with or without a small patch of surface granulations, which dried up slowly. There was no pus given out, except from the surface granulations, and its amount was insignificant. The eighth case made an excellent recovery after exfoliation of a portion of the tibia. Suppuration was delayed for nearly three weeks, and the wound continued antiseptic after its occurrence.

Two compound fractures of the skull healed without any

suppuration. In one there was depression, and the trephine was used. In the other the outer table only was ascertained to be somewhat deeply indented.

Several deep-seated collections of pus were emptied antiseptically, with most satisfactory results. One man made an excellent recovery after a severe illness caused by an abscess in the left kidney. An equally good result followed in two instances of large abdominal abscess, caused by pelvic cellulitis after parturition.—*Lancet, Jan.* 14, 1871, *p.* 47.

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## 110.—GLYCEROLE OF STARCH.

This preparation is made by rubbing well together one part of starch in eight of glycerine; then heat the mixture gradually to 240° Fahr., constantly stirring until a translucent jelly is formed. The glycerole of starch is a capital substitute for lard in making ointment. Moreover, this preparation of starch seldom becomes spoiled, and keeps for a very long time. As a local remedy in many acute affections of the skin, and to prevent the pitting of small-pox, it deserves a more extensive trial.—*Journal of Cutaneous Med., Dec.* 1870, *p.* 235.

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## 111.—SOLUTION OF SANTONINE.

By Dr. JOHN HARLEY.

The insolubility of this vermifuge impairs its utility. Cold or warm water takes up the merest trace. Chloroform, absolute alcohol, the strongest acetic acid, turpentine, hot olive oil, and hot glycerine are the only simple fluids that dissolve any appreciable quantity. On cooling, it separates from the oil and glycerine; and the addition of water to the other solvents produces the same result.

It is obvious, therefore, that none of these solvents are adapted for the use of santonine as a medicinal agent. A wish to determine the effect of santonine in parasitic disease of the bladder led me, after a good deal of trouble, to find that I could form a suitable stronger solution than was needed for my purpose by means of carbonate of soda.

I may formularize my results thus:—

℞. Santonini, in pulvere, gr. xij.; sodæ bicarbonatis, gr. xx.; aquæ destillatæ ʒiij.

Put the soda and water into a flask, keep the fluid near the boiling-point, adding, as it disappears, about two grains of the santonine at a time, until the whole is dissolved. Solution is effected in about half an hour, during which time the water is reduced to ʒij. If need be, reduce by boiling to this bulk, when ʒj will contain a full dose—six grains of santonine. If

As a *plaster*, one part of carbolic acid to three parts of shellac.

The crystallized carbolic acid to be used as a caustic.

The carbolate of glycerine, as above, use in one or two drop doses, internally.

Antiseptic oil for abscesses, one part of acid to four of boiled linseed-oil.

Antiseptic putty, six spoonfuls of the antiseptic oil mixed with whiting.

Aqueous solution of carbolic acid is one part of acid to forty of water (one ounce of acid to a quart of hot water well agitated and filtered).

Sick-rooms, to disinfect: place a portion of the dissolved crystals in a porcelain dish, and float it in a larger vessel of hot water.

Disinfecting purposes generally: one pound of *crystals* to six gallons of water. *Fluid*, one part to eighty of water. *Powder*, one ounce of crystals with four pounds of slaked lime.

For drains: one pound of the fluid carbolic acid to five gallons of warm water.

Toothache is often cured with one drop of carbolate of glycerine; and diarrhœa arrested in half an hour with two drops in a wine-glass of water.

In all cases of parasitic life it is advisable to commence with very dilute carbolate of glycerine.

Inasmuch as carbolic acid will destroy the power of *vaccine virus*, it becomes an interesting inquiry as to the possibility of using carbolic acid internally as a preventive, so as to fortify the human system against the incoming of zymotic diseases.

I have some striking facts in support of this probability, but my observation has been too limited to do more than incline to the belief; and here I leave it in the hands of the Conference, expressing my readiness to give further details if the members desire it.—*Edinburgh Med. Journal*, *Dec.* 1870, *p.* 563.

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## 114.—KALI-KUTKI: A NEW TONIC.

Mr. M. C. Cooke, in a late number of the Pharmaceutical Journal, gives a description and an account of the properties of this plant, which has long been considered to be the black hellebore, but which really belongs, not to the Ranunculaceæ, but to the Scrophulariaceæ. Though unknown in the English market, it is well known throughout India. The drug consists partly of the root and partly of the stem of the plant. The root part is very light and brittle, about the size of a goose-quill, brownish-white in colour externally, and deep black

internally, with short waxy fracture. It is stated to be a very valuable tonic, Assistant-surgeon Moordeen Sheriff considering it equal to gention and calumba, and superior to chirayta. As a dose, ten or twenty grains as a tonic, and from twenty to forty as an antiperiodic, are recommended.—*Lancet, Feb.* 4, 1871, *p.* 169.

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## 115.—CLEARING OF MUDDY WATER.

Dr. C. Schlœsing states, in an article in the Comptes Rendus, that waters contaminated by floating particles of clay may be readily clarified by small quantities of salts of lime. It is well known that the waters of rivers, after a heavy fall of rain or snow, and sometimes throughout the winter, do not become quite clean by deposition, even if left undisturbed in large reservoirs for a long space of time. The author recommends the addition of 1-1000th part of chloride of calcium for one part of water (or seventy grains to the gallon), a quantity which effects clarification in a moment. The precipitated substance can be readily separated by filtration. Other salts of lime, such as the nitrate and bicarbonate, and caustic lime, effect the same object.—*Medical Press and Circular, Nov.* 30, 1870, *p.* 453.

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## 116.—A PLEASANT REMEDY FOR SEA-SICKNESS.

There have been many suggestions made as to the prevention of sea-sickness, none of which have, to say the least, been found in practice to be completely successful. The introduction into practice of hydrate of chloral, which produces with certainty sleep for a definite number of hours, has suggested a means of escaping the horrors of a short sea-passage at least, and possibly of mitigating the most prolonged horrors of sea-sickness. To go asleep at Dover, and to wake to find oneself at Calais, is a plan which, failing other expedients, has in it much promise. An ordinary dose of hydrate of chloral produces sleep usually in a quarter of an hour, and with almost unfailing certainty. Some cases just published by Dr. Doring, of Vienna, seem to show that the value of hydrate of chloral to obviate sea-sickness is very great. It produces quiet and prolonged sleep. In all the instances recorded, it seems to have been of great value even during prolonged sea-voyages, giving a good night's rest, arresting violent sickness when it had set in, and stopping the tendency to its recurrence.—*British Med. Journal, Dec.* 3, 1870, *p.* 608.

## 117.—ON A RAPID AND ACCURATE METHOD OF MILK ANALYSIS; WITH SPECIAL REFERENCE TO THE EXAMINATION OF WOMAN'S MILK.

By JOHN MUTER, Ph.D., Director of the South London School of Chemistry and Pharmacy.

In entering upon this subject, it will not require any preliminary verbiage to show the necessity of keeping a watch on the composition of the nursing mother's milk. It has been often a matter of wonder to me that medical men did not avail themselves more frequently of the facilities offered by an analysis of the nurse's milk as an aid to the diagnosis of obscure cases of infantile disease, such as persistent diarrhœa, or continual loss of flesh without any visible cause. In such cases I have heard medical practitioners state their conviction that the milk was wrong; but when I asked them in what direction the fault existed, they were at a loss for a reply, and when I suggested an analysis they seemed inclined to think little of it, as they had never before dreamt of employing such an aid to diagnosis. On going more deeply into the subject, I found that several great impediments existed to the general employment of analysis, such as the quantity required, and the tedious and not over-accurate process of milk-examination still in use. It was therefore clear that, to render chemistry a useful aid to medical science in this matter, we ought to have a process which would combine smallness of sample, rapidity, and fair accuracy.

After many trials in various directions, it was determined to base the analysis on the real dietetic value of the milk, as represented by a determination of its solid residue, and the estimation of its nitrogenous and carbonaceous constituents. Experience derived from the use of Dr. Frankland's elegant system of water analysis by combustion *in vacuo* led ultimately to the application of a similar principle to milk. The process, when used for cow's milk, requires several modifications, in view of possible admixture with certain extraneous matters; but as it is only with woman's milk that I have to do in the present paper, these additional precautions need not now be mentioned.

The process is conducted as follows:—A small paper filter is prepared which will contain 10 grammes of oxide of copper. It is filled with the oxide and placed in a small funnel. The whole is then exposed for an hour in an air bath, so constructed as to draw a continuous current of air at 105° C. through and around the funnel. The funnel and its contents are then cooled in the desiccator, and weighed with great care, and the weight having been noted, *five drops* of the milk are cautiously let fall on the centre of the oxide, taking care that

no milk spreads to the filter paper. The whole arrangement is then weighed, and the quantity of milk used found by difference. The funnel and contents are then replaced in the air bath, and exposed to a temperature gradually increasing from 50° to 105° during several hours. When the milk has been thus dried it is again weighed at intervals until it ceases to lose weight. This loss of course shows the percentage of water, and, consequently, of the total solids. The contents of the filter are now transferred to a warm glass mortar, mixed with more oxide of copper, and introduced into a combustion tube. The tube is charged in the usual way, but the front part is filled through three and a half inches of its length with a roll of copper gauze previously ignited in hydrogen. The end of the combustion tube is then drawn out before the blow-pipe, and connected by an india-rubber and glycerine joint to the Sprengel air-pump. Heat is now applied to the front part of the tube, and, the air having been fully exhausted, the combustion is proceeded with, and the residual gases received into a tube over mercury. At the close of the combustion the gases are entirely transferred from the combustion to the receiving tube by the aid of the pump, and it only now remains to measure them in a manometric apparatus. For this purpose I employ the apparatus described by Mr. M'Leod in the Journal of the Chemical Society, and I find it to work most satisfactorily. The gases are first measured as a whole, and then separately absorbed by the usual reagents and measured after each absorption. We thus obtain the total volumes of carbon dioxide and nitrogen, evolved by the milk-residue, and by a simple set of calculations we are enabled to deduce the actual weight of carbon and nitrogen contained in it, and consequently its exact value in nutriment. In one continued series of operations we therefore ascertain (1) the total solids, and (2) the amount of their flesh-forming and heat-producing constitutents. When thus complete, the analysis would be expressed as follows:—

*Example I.*

Milk from E. S., one month after confinement; child healthy.

Amount of milk taken, 0·24 grammes.

Total solids, 0·0264=11·00 per cent.

Organic nitrogen .. .. ·0014=0·5833 per cent.

„ carbon .. .. ·0167=6·5420 „

Therefore—

| | |
|---|---|
| Water .. .. .. .. .. .. | 89·00 |
| Flesh-formers (casein) .. .. .. | 3·71 |
| Heat-producers (fat and sugar) .. | 7·29 |
| | 100·00 |

This is a good specimen of well-balanced, healthy milk.

*Example II.*

A. B., time after confinement not mentioned; child ailing; diarrhœa.

| | |
|---|---|
| Water .. .. .. .. .. .. .. | 87·69 |
| Flesh-formers (casein) .. .. .. .. | 0·91 |
| Heat-producers (fat, &c.) .. .. .. | 11·40 |
| | 100·00 |

A milk so poor in nitrogen as to be worthless for the due nourishment of the infant, while there is immense excess of sugar and fat.

Having thus laid this plan of analysis before the profession, it is for them to consider the clinical value of such examinations. It is not for me, as a chemist, to point out this further than to ask whether, by proper investigation into the properties of its mother's milk, the sick child might not often be saved by applying to her the remedies and, above all, the course of diet shadowed out by such analysis, or by at once altering the mode of feeding the infant altogether? There can never be any difficulty in procuring ten drops of the milk as a sample, and the whole analysis can be usually done in one day. Should these preliminary remarks attract the attention of any gentleman who has cases suitable for the process, I shall be glad to join him in extending the field of inquiry.—*Lancet, Feb.* 11, 1871, *p.* 189.

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## 118.—HOW TO APPLY LEECHES WITH LEAST TROUBLE AND ANNOYANCE, AND GREATEST EFFICIENCY.

By L. R. (a Correspondent of the *Lancet.*).

Having had occasion to order a mustard-poultice for a patient, it became requisite to put some leeches on the same place. I was told that they fastened instantly, filled rapidly, and that the blood streamed afterwards into bread-poultices as if it would never stop. I took the hint; and now, whenever I order leeches, I always have a mustard-poultice applied first, then the leeches (two or three instead of half a dozen), and then bread-poultices. There is less trouble for those who have to apply the leeches, far less annoyance, weariness, and exhaustion for the patient, and a much more satisfactory result. The flow of blood is, however, sometimes so much greater than would be thought likely or possible, that I think it right to add a few words of caution. A few days ago one of my patients, a young lady, grown up, and of average strength, bled to fainting from only two leeches applied in this way.—*Lancet, Dec.* 10, 1870, *p.* 840.

## 119.—GLYCERINE INHALATIONS IN CROUP.

The Wiener Medizin Wochenschr, for November 19th, 1870, gives an analysis of a pamphlet by Dr. G. Stehberger, of Mannheim, who recommends the treatment of croup by inhalation of pure glycerine through Siegle's apparatus. He was led to try this remedy in croup from observing its good effects in cases of hoarseness and loss of voice. In 1869, after an epidemic of measles, there were numerous cases, in the practice both of Dr. Stehberger and of other practitioners in Mannheim, where, whether the symptoms were those of true or of false croup, the good results of the inhalation soon became evident. The cough became more free and moist, and the children were able to sleep almost immediately after being relieved by the inhalation. In severe and advanced cases, however, these results were not so evident; and it is doubtful whether they occur, if the remedy be not applied early and repeated sufficiently often. The glycerine is used unmixed, if it be pure; if not pure, it is diluted with a little water. The inhalations are repeated, according to the urgency of the case, at intervals varying from half an hour to an hour and a half, for about fifteen minutes at a time. Dr. Stehberger ascribes the effects of the glycerine to the fact, pointed out by Dr. M. Sims, that it increases the secretion of the mucous membranes, and thus reduces tumefaction.—*British Med. Journal, Jan* 14, 1871, *p.* 39.

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## 120.—ODONTALGIA.

Dr. H. T. Reynolds writes to the Phil. Medical News as follows: "For eighteen months I have been using acetate of lead for toothache. I find it to act better than any of the numerous remedies proposed in the books, and in cases in which it is applicable the relief is instantaneous. Let the patient apply one or two grains to the cavity and then spit it out. It fails in fewer cases than any remedy I have tried."—*Medical Record, Dec.* 15, 1870, *p.* 467.

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## 121.—A SUGGESTION FOR THE PREVENTION OF INFECTION.

By JAMES STARTIN, Esq., Senior Surgeon to the Hospital for Diseases of the Skin, Blackfriars.

The rapid increase and extension of small-pox, scarlet fever, and other infectious diseases at the present time, not only in those localities which may be designated their habitual haunts, but also within the wards of general hospitals, and through the agency of public vehicles, laundries, &c., induces me to offer a

suggestion, which, during twenty-five years, I have been in the habit of recommending for arresting contagion in diseases of the skin. According to my knowledge, the proposition has not hitherto been employed against fevers and other sources of infection or contagion, although I am aware that it has been used in the arts for sweetening, deodorising, or disinfecting wine-casks, dairy utensils, &c. The suggestion consists in the simple expedient of fumigating beds, clothing, closets, carriages, &c., with sulphurous acid gas, according to the following "ready method." In the case of disinfecting beds and bedding, five to fifteen minutes before the patient enters the bed, or during his removal whilst it is made, a copper warming-pan, containing a few live embers, on which a teaspoonful or two of flowers of sulphur have been thrown, is to be introduced between the sheets, and passed to and fro until the combustion of the sulphur is completed, when the pan is to be withdrawn; and, after the lapse of a few minutes, the patient may enter the bed, when, should the fumes still prove too stimulating for his respiratory organs, these vapours may be intercepted by holding a loosely folded damp handkerchief before the nose and mouth until they have subsided. In localities where a copper warming-pan is not procurable, a common wicker hand-basket, or a small hamper, containing an old iron saucepan or flower-pot, or basin, in which a few hot cinders have been placed, may be substituted. This, like the warming-pan, must be put beneath the bedclothes, and moved about during the ignition of the sulphur, until the sulphurous acid gas has sufficiently permeated the linen, &c., to be disinfected.

It will be perceived that this latter plan of using the sulphur may be adapted to fumigating closets, carriages, passages, and, indeed, to the vacated chambers of the sick; the only precautions to be borne in mind being to take care that the quantity of sulphur or heat of the live embers be not too considerable, and that the wickerwork is of sufficient height and capacity to prevent the articles fumigated from being burnt in the operation.

With regard to disinfecting the clothing, articles of dress, &c., these should be lightly sponged over or sprinkled with water containing a little *well mingled* milk of sulphur, in the proportion of a teaspoonful of sulphur to each pint of water. The articles should be then ironed by means of a flat iron heated to a sufficient temperature to volatilise the sulphur, but not to burn the clothing. Of course repetitions of this process will be required, according to the extent and duration of the infection.

The daily journals have lately contained an account of a cabman who was fined £3, the cost of disinfecting his cab, after the carriage of a small-pox patient. By the means suggested,

of sponging the cab-lining with the sulphur and water, and afterwards using the hot flat iron, and perhaps adding the basket-fumigation, about as many pence would be the cost of the disinfection.

It has been urged as an objection to the above procedure, that it might prove injurious to health, from the noxious vapours of carbonic oxide given out. I need scarcely observe that this objection falls to the ground if the directions given be faithfully carried out; as, if the live embers be properly incandescent, and not smoky, the sulphurous acid gas only will be produced.—*British Med. Journal, Jan.* 21, 1871, *p.* 60.

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## 122.—ON DR. VIVODTSEF'S METHOD OF EMBALMING THE DEAD.

By Dr. GEORGE L. CARRICK, Physician to the British Embassy at St. Petersburg.

[Embalming the dead, so largely resorted to by the ancient Egyptians as a sanitary measure, is now so rarely practised that the majority of medical men have never witnessed the process, nor observed its results. The difficulty and tediousness of the operation, and its frequent failure in many instances, necessarily rendered it a very expensive one, so that it has only been called for in very exceptional cases. An easy, quick, and cheap method of embalming has been long sought for, but until Dr. Vivodtsef's invention the chief difficulties remained unsurmounted. Dr. Vivodtsef's process is so simple, short, and easy of accomplishment, and requires so little skill, that any person possessed of the slightest knowledge of practical anatomy can execute it as easily as the most experienced dissector can, and, consequently, it need never, of course, be expensive. If correctly carried out, it will not be followed by decay of any part of the body embalmed.]

In all former modes of embalming there were faults in the fluid injected, as well as in the mode of injecting it. Dr. Vivodtsef's method differs from all others—1st, In the kind of material used; 2nd, In the manner of injecting it; 3rd, In leaving the brain and thoracic and abdominal cavities intact.

We shall now consider *Dr. Vivodtsef's Method of Embalming*, which consists in injecting into the arteries, by means of Dr. Richardson's apparatus for local anæsthesia, a mixture of carbolic acid and alcohol. For the successful performance of the operation, the following materials and armaments are needed:—1st, Alcohol of about 90°, holding in solution one-fifth its weight of carbolic acid. The quantity of mixed fluid must equal about one-half the weight of the body. 2nd, A glass

bottle (Fig. 1) capable of holding from five to fifteen pints of the above-named solution. This bottle is provided with an indiarubber stopper, which has two glass tubes, a long one and a short one, running vertically through it. The long tube (A) passes down to the bottom of the bottle, while the other and short one (B) only reaches the lower surface of the stopper. The upper ends of both tubes, after rising an inch out of the stopper, are bent at right angles to it, and then run horizontally in opposite directions. (See woodcut, p. 385.)

The upper extremity of the long glass tube is connected by means of about two feet of indiarubber tubing (C), with a fork-shaped copper tube (D), on the two branches (F) of which two indiarubber tubes (X), several inches in length, are slipped. Each of these tubes again has a fork-shaped metallic tube (G and G) inserted into it, and to the free ends of both metallic tubes, four indiarubber tubes (E), from six inches and upwards in length, are attached. These four tubes terminate in the small metallic canulæ, which are inserted into the arteries, and through them the fluid passes into the body. The four canulæ (H) are small brass tubes, T-shaped (Fig. 2), and varying in size according to the size of the body, or rather of the artery, we desire to inject. In the apparatus employed for embalming adults, the shaft (*a*) of the tube is about four times the thickness of its branches (*b*, *b*), which vary in breadth from the sixteenth to the tenth of an inch. These branches are inserted into the artery, which is incised longitudinally to admit them. Each branch has a small groove (*c*, *c*) running round it, and into this groove the string used for tying the artery to the canula slips, and thus keeps the injected fluid from regurgitating.

We now pass to the consideration of that part of the instrument that is attached to the short glass tube (B), through which air is pumped into the bottle with a pressure sufficient to send the fluid through the long glass tube into the arteries.

The atmospheric pressure is accomplished by the same apparatus (only made much larger) which Dr. Richardson employs for producing local anæsthesia, and consists,—1st, Of a thick-walled very elastic indiarubber bag (K), ellipsoid in shape, about eight inches long and three wide. At its free extremity it is provided with a valve (V) which opens from without inwards. Its other end connects it, by means of elastic tubing (O), with 2nd, An indiarubber ball (L), through the middle of which passes, and to which is attached, a copper tube (N) with numerous holes at its sides. The other extremity of the indiarubber ball is connected, by means of elastic tubing (P), with the short glass tube of the bottle. This is all the apparatus required for embalming.

The process is the following:—Firstly, the urine is drawn off

with a catheter, and the rectum and descending colon washed out by means of warm water. The common carotid, brachial, and femoral arteries on both sides are then cut down upon, and their accompanying veins—the jugular, brachial, and femoral—exposed to view. A longitudinal slit, of about half an inch in length, is then made into the anterior surface of both common carotid and brachial arteries, and both jugular and brachial veins. The small T-shaped metallic canulæ are then introduced into the open mouths of the four arteries. This is accomplished by pushing into the artery, through the incision made in it, first one branch of the canula and then the other.

Having introduced both branches in such a manner that the upper part of the shaft (now turned downwards) occupies the middle of the slit, two ligatures are passed round the artery—one ligature about an eighth of an inch from one end of the opening, the other the same distance from the opposite end of the opening. Each ligature is then tightened and tied, and embraces that part of the artery which covers the small groove passing round the branch of the canula (Fig. 3). The ligatures are placed to keep the canula in its position, and to prevent the fluid regurgitating through the slit in the artery. All the four arteries are secured in the same manner, but the openings in the veins are as yet left untouched.

The glass bottle, filled with the antiseptic liquid, is next placed on the table, while the ellipsoid bag and indiarubber ball are laid on the floor. The bottle is then connected with the four canulæ (now inserted into the arteries) by means of four elastic tubes (E). The connexion between the injecting apparatus and canulæ being now established, the operator places his foot on the elastic ellipsoid bag (K), squeezes the air out of it, and allows it to refill by the removal of his foot. Every time he presses on the ellipsoid bag he fills the indiarubber ball (L), whence the compressed air passes along the indiarubber tube (P), and then through the short glass tube (B) into the bottle. The compressed air presses upon the contents of the bottle, and sends the fluid, first through the long glass tube (A), then through the fork-shaped metallic tubes, and through the indiarubber tubes, and, lastly, through the canulæ into the arteries. The fluid travels along the arteries in two directions, upwards and downwards. A continuous current is thus kept up, and a gradual diminution in the contents of the bottle is soon observed. When the body begins to swell and the face to get puffy, particularly under the eyelids, the canulæ should be withdrawn, and the arteries tied at both ends of each opening. So far, however, only the carotid and brachial arteries have been injected. The operator must now proceed with the two femoral arteries, and as merely two canulæ are here needed, the

four elastic tubes (E), as well as the two forked metallic tubes (G), are removed from the apparatus, and the two canulæ are inserted into the two elastic tubes (X). The process of injection is again commenced, and continued until clots of blood at first, then blood mixed with the antiseptic fluid, and, lastly, the fluid alone, escape from the opened veins. The veins are now ligatured, and the pumping of the solution into the body is continued, until an opaque, yellow brown fluid, a mixture of blood and mucus, begins to flow from the nostrils; while at the same time the operator feels the ellipsoid bag resisting the pressure of his foot. This is a sign that the viscera and blood-vessels of the body are already full, and can contain very little more liquid. The injection is continued, however, for a minute or two longer, until a clear fluid commences to escape from the nostrils, and the ellipsoid bag no more yields to pretty firm pressure.

The operation is now brought to a close. The canulæ are withdrawn, the femoral arteries ligatured in the same way that the carotids and brachials were, and all the wounds are carefully and closely sewn up. It should be remembered that in embalming the body of a child it is sufficient to inject the carotid and femoral arteries only; in an infant the femoral alone.

*Changes which the Body undergoes after Embalming.*—Towards the termination of the process of embalming, the post-mortem rigidity and cadaveric lividity gradually disappear, the abdomen swells considerably, the chest assumes a rounder form, the face fills up, and the eyelids become puffy, as in acute albuminuria. The skin, particularly of the face, ears, and neck, loses the waxy and semi-transparent appearance it so often presents after death, and becomes whiter and more opaque, feeling like parchment to the touch.

In a few days, however, the puffiness of the face and eyelids disappears, and the features assume their natural proportions. The parchment-like condition of the skin is permanent. The body continues in this state, without undergoing any further alteration, for three, four, or six months; after that it begins to shrivel somewhat, the features become sharper and more pinched, the cheeks fall in, the eyeballs sink deeper into their sockets, the abdomen and chest decrease in circumference, and the extremities become palpably more bony. The skin still retains its white colour, however. After six months—although sometimes not until after eight, or ten, or even twelve—it begins to get dusky, and then its tint gradually deepens until it assumes a decidedly brown colour. The soft parts now shrink more and more, and the whole body becomes thinner and drier; in other words, it begins to mummify. In this mummified condition (in which, however, the muscles are soft and flexible) it may last for almost any number of years.

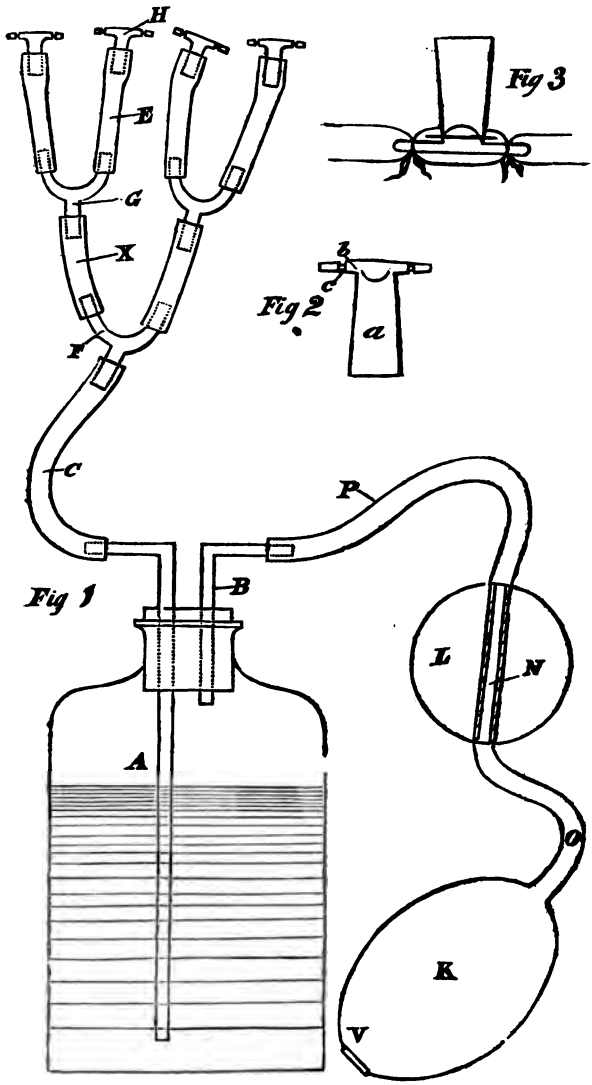
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Fig 1
Fig 2
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Fig 3

*The Time required for the Process of Embalming* will of course depend much upon the skill and energy of the operator, the assistance he has at hand, and the condition of the subject he works upon. Thus, if the materials be all ready; if two bottles, each connected with a separate injecting apparatus, be employed, so that the injection of femorals, carotids, and brachials can be carried on at the same time; if the body be lean; and if the operator be skilful in cutting down upon the arteries; and last, not least, if he have a smart assistant—the whole operation may be completed within an hour. Usually, however, from two to three hours are needed, sometimes even five or six, from the commencement to the end of the operation. Infants are more quickly embalmed than children, children than adults.

*Points to be attended to in Embalming.*—1. The fewer and the smaller the incisions made the better, as every cut in the skin allows of some portion of the injected fluid to escape through the severed capillaries during and after injection. 2. It is necessary to keep several veins open until all the blood escapes from the body, and then to tie them with a double ligature as soon as the antiseptic fluid begins to flow from their cut surfaces. 3. The fresher the body the more successful will the operation prove. When signs of decomposition have made their appearance—as, for instance, green patches on the abdomen—success is doubtful. 4. The injection should be stopped then only, when the ellipsoid bag strongly resists pressure. 5. It is well, although not absolutely necessary, to inject some of the antiseptic fluid into the rectum, and colon, and bladder.

*General Remarks.*—1. Fluid escapes from the nostrils for several hours, and sometimes even days, after embalming. 2. The antiseptic fluid runs pretty largely from the incisions made in the skin, particularly if the wound is not closely and well stitched. 3. There is a very strong smell of carbolic acid, which smell continues for many days after embalming. 4. The operator should have gloves on when using the injection, as carbolic acid is extremely corrosive, and quickly destroys the cuticle. Thus, even when its action has been slight on the epidermis, it takes weeks before the skin loses its roughness, the hands always presenting the appearance of being unwashed. 5. Post-mortem rigidity and cadaveric lividity disappear during embalming. 6. When the operation is over, the cheeks and lips of the corpse should be slightly touched with rouge, so as to give the face a more natural appearance.

*Cost and Uses of Embalming.*—The price of Dr. Vivodtsef's apparatus for embalming is from 20 to 30 shillings. The fluid (if the alcoholic solution be employed) must be estimated at, on an average, two shillings the pound avoirdupois; and of this

fluid an amount equal to at least one-third the weight of the body will be required. Thus, to embalm a person of 12 stone, the cost of material would amount to about £5 12s.; to embalm a child of fourteen would cost about £2 15s.; while an infant up to its second year could be preserved for about 30s. If the carbolic acid, as lately used by Dr. Vivodtsef, be dissolved in equal parts of water and glycerine instead of pure alcohol, the price of embalming would be one-half, if not less, of the above estimate, as the glycerine need not be of the best quality.

The uses of a cheap and easy method of embalming are threefold. 1st. It will enable people to procure the bodies of those of their relatives who have died away from home free from decay, and presenting hardly any post-mortem change. 2nd. It might, as a hygienic measure, be advantageously introduced into large towns, where, as in Edinburgh and Glasgow, the cemeteries are situated in the very centre of the city, and where the dead are placed in family vaults instead of being regularly interred. 3rd. It will prove of service to anatomists, as bodies can thus be kept for many months in the dissecting-room, without the muscles, bloodvessels, or viscera undergoing any anatomical change.—*Edinburgh Medical Journal, Dec.* 1870, p. 505.

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## 123.—ON THE AFTER-TREATMENT OF CATARACT.

By HAYNES WALTON, Esq., Surgeon to St. Mary's Hospital.

The very first act after the operation should be to close the eyelids with a couple of strips of court-plaster, about an inch and a half long and a quarter of an inch wide. This insures adaptation of the wound, and supports the corneal flap. Besides this, the atmosphere is excluded, and the eye is rested. It is the simplest appliance by which the eye can be kept closed, and such closure accomplishes all that can be done for the wound without disadvantage. The tears and the aqueous humour readily escape, because some portions of the edges of the eyelids are uncovered. Bandages and compresses of all kinds are injurious. They are hurtful in proportion to their action. All pressure, beyond that which is naturally produced from closing the eyelids, must tend to be prejudicial, and, at times, to be positively damaging. Again, whatever keeps the eye hot must be bad—whatever soaks up the secretions is objectionable. I am well aware, from personal observation, that some Surgeons pack the eye with cotton wool or charpie, and subsequently apply a bandage. The system is very like the French method of treating a stump after an amputation. It is notorious how a French Surgeon packs and bandages until the limb has pounds of material over it. I fully believe

that my plan ensures the highest attainable result that can be reached.

Whenever it seems necessary to protect the eye from blows from the patient's own hands, a stiff square shade, reaching from one temple to another, padded where it touches the head, and kept in position by an elastic band, will suffice. Calkin's eyeshade answers the purpose very well.

I believe it to be important that the patient be well fed, and therefore I allow him a full diet. Of course, he is carefully fed by the nurse, and does not make any exertion. It is a mistake to prescribe liquid food under the idea that chewing is hurtful to the eye, for in man the muscles of mastication cannot in any way influence the eyeball. It is different in most of the lower carnivora.

From day to day the corners of the eyelid and the cheek should be carefully cleansed. The patient should be kept in bed for the greater part or the whole of the week, but not necessarily between the sheets. When the bed is left, there is always a risk of imprudent acts. You must have observed that the principles of treatment of my ophthalmic operations differ in nowise from those of my operations in general Surgery. When I amputate a leg I do all I can to secure adaptation of the flaps, and to insure rest, by applying a splint under the limb, and I am scrupulously careful to avoid all unnecessary disturbance, all strain on the parts that are placed in apposition. Very little movement will spoil the healing of the eye or the limb.

When seven clear days have passed without unfavourable symptoms, success is pretty certain, and the plasters may be removed after they have been thoroughly softened with warm water. Then I try the sight by putting the patient's back to the light, and guarding the eye while he looks at something held before him. At the same time I endeavour to ascertain whether the cornea has healed. If the chambers of the eye be filled with aqueous fluid, union must have taken place. When everything is satisfactory the plasters need not be reapplied, but if otherwise, they are required. There may be simply delay in the healing, in which case the eyelids require to be closed as much as at first.

The pernicious practice of opening the eye a few days after the operation cannot be too strongly deprecated. The examination is quite useless if the eye be doing well; if otherwise, it is certain to aggravate any evil, and in no instance can it disclose symptoms for guidance more certain and more valuable than those of the patient's sensations and the state of the eyelid. A red and puffy eyelid is a sure indication of an inflammatory state of the eyeball, with an absence of proper repair

in the corneal wound. Besides this, the angle of the cheek gets œdematous.

This after-treatment, which may be said to be purely mechanical, is really all that can be done for a patient; and when the cornea does not heal kindly, the case is never perfectly successful, do what you may, and in most instances the eyeball is somewhat damaged, and only an imperfect result as regards vision is obtained. The very best sight is never got except the pupil be round and central, or nearly central. It is not enough for a patient to be able to read the smallest type under certain favourable conditions of light and position; he may do this with an irregular and displaced pupil. I want more: I wish him to be able to face a bright light without distress, and to get the light admitted to the centre of the retina, for both of which the pupil must be of the natural size, or nearly so, and central.

I will mention concisely the unfavourable conditions which may ensue:—Hemorrhage from the interior of the eyeball, irrespective of any loss of the vitreous humour; non-union of the cornea, in part or in whole, with prolapse of the iris; the giving way of the cornea after it has healed; acute inflammation of the eyeball, as a direct consequence of the operation; sub-acute inflammation coming on several days after the operation, which is far more common than the acute; inflammation of the entire eyeball, with suppuration. Each of these conditions require careful study and dissimilar treatment. It is not my intention to dilate on them. I may say, in a few words, that nothing is gained for the eye by violent antiphlogistic measures. When any of the untoward states which I have named exist, all chance of primary union of the cornea is gone, and whatever depresses the patient will very materially tend to lessen the prospect of his recovering any degree of sight. He is sure to be enfeebled by the adverse condition of the eye, and the constitutional symptoms are often severe. It may be said, in general terms, that only those measures which tend directly to relieve pain and to soothe are admissible.. With the least chance of saving the eye, the therapeutic measure most to be relied on is rest to the wounded part—that is, the eyelids must be again plastered. This may be necessary for weeks. Many an eye is lost because this is not attended to. All else is in vain if this be neglected. I believe its simplicity is the chief reason why this, my practice, is not more generally followed. It leaves nothing for meddlesome fingers to do. While in the first week I always plaster up both eyes, in the secondary treatment, to which I now allude, I close only one. I never perform double extraction at once: my reasons against it have been given.—*Med. Times and Gazette, Dec.* 3, 1870, *p.* 637.

## 124.—OBSERVATIONS ON THE SKIM-MILK TREATMENT OF DIABETES MELLITUS.

By Dr. ARTHUR SCOTT DONKIN, Lecturer on Medical Jurisprudence and Toxicology to the University of Durham.

[Dr. Donkin's first article upon this subject will be found at page 104, *Retrospect*, vol. lxi. The subject is so interesting and important that we are surprised no further notice of it has been taken by the profession, than a short one by Dr. George Balfour of Edinburgh. (See *Retrospect*, vol lxi., p. 107.) In the case treated by him the improvement is stated to have been "very remarkable," but the patient only remained under his care a few days, and the treatment was not fairly tried. The fact is extraordinary that both the following cases were *cured* within fourteen days after beginning the treatment.]

*Case* 1.—*Diabetes mellitus; removal of the sugar from the urine in fourteen days; complete recovery.*—Mr. J. G., aged fifty-eight, a highly respectable merchant, of large, robust, muscular build, and of regular and temperate habits. He has devoted himself very successfully to business pursuits, with all the anxiety attendant thereon.

For two years prior to May, 1870, this patient had grown much stouter and shown a decided tendency to corpulency, and, to use his own expression, "had been very bilious." He suffered much during this period from loss of energy and fatigue on exertion, always feeling dull, heavy, and languid, sleepless at night and drowsy in the daytime. This general indisposition he attributed to his habits having become much more sedentary and his application to business much closer. He took no stimulants whatever during the daytime, but in the evening, at dinner and after it, he took daily a pint of bitter ale and one or two glasses of whisky, but never more than this quantity.

This condition of general debility and suffering, just described continued until the beginning of May, 1870, when the patient's health completely broke down, and his feeling of debility increased so greatly that he could with extreme difficulty walk in the morning after breakfast between his own residence and his place of business, a distance of about half a mile.

This condition continued until the 14th of June (about six weeks), when I was called in consultation with Mr. M. Francis, surgeon to the Sunderland Police Force, when we found the state of the patient to be as follows:—He was unable to attend to business, and could not walk more than a quarter of a mile without taking a rest. He suffered much from a dull heavy pain across the forehead, with a painful dragging sensation in the face, or as if something was dragging down his cheeks. There was great dimness of vision and loss of energy, no sleep

at night (although he slept occasionally towards morning), coldness and numbness of the limbs, and loss of sensation on the anterior surface of the thighs. There was not excessive thirst nor inordinate appetite; the skin was somewhat dry and not perspiring; the gums were spongy, and the teeth loose. There was nothing abnormal in the state of the pulse, and no organic disease could be detected.

Such were the general symptoms; and a careful examination of the urine revealed the nature of the disease. The daily quantity of urine varied from eight to ten pints; and its specific gravity was from 1035 to 1040, and it was loaded with sugar. The case was, therefore, unquestionably one of diabetes mellitus.

On the following day (the 15th of June) the skim-milk treatment was begun; and, on account of the large muscular frame of the patient and his sharp appetite, from eight to ten pints were taken daily, and at the ordinary temperature. The cream was taken off carefully after it had stood a sufficient length of time, and according to directions. *All other food was scrupulously refrained from;* and no medicine whatever was in this case prescribed. This treatment (an exclusive diet of skim-milk was persevered in *without variation or intermission for a period of five weeks.*

And now let us consider with what result.

At the end of the first week of the treatment, the urine had fallen in quantity to six or seven pints daily (the quantity being always in direct ratio to that of the daily consumption of milk), and its specific gravity was reduced to 1015, the quantity of sugar having undergone a very great diminution.

At the end of the second week (June 28th), or *fourteen days after the commencement of the treatment, the sugar had completely disappeared from the urine;* not the slightest trace could be detected on the most careful examination. The specific gravity of the urine was now reduced to 1009 and 1010 daily, the quantity ranging from six to seven pints according to the quantity of milk taken daily; but whenever the quantity exceeded seven pints the specific gravity was always below 1010. From this date the sugar continued absent from the urine.

As regards the general symptoms of the disease already enumerated (most of which were referable to the nervous system), they gradually diminished, and at length completely disappeared within a fortnight, and were succeeded by a feeling of perfect health, accompanied by profound refreshing sleep at night. The lethargy, too, entirely disappeared.

One of the most remarkable changes produced in the patient's condition was the restoration of his strength; *at the end of a month he walked seven miles without once resting, and without fatigue* or subsequent injury; his diet having been all the while exclusively skim-milk, as already stated.

At the commencement of the treatment the patient was flabby and inclining towards obesity; but two months afterwards his flesh was firm and compact, his features presenting a ruddy healthy hue, instead of his previously yellow, pasty look; his gums and teeth regained their firmness.

As already stated, the skim-milk treatment was continued for five weeks. At the end of this period from two to three pints of the daily allowance of milk (from eight to ten pints) were converted into curd by the use of Proctor's essence of rennet; this curd was taken at two meals, and it assisted materially in filling the stomach, and was thus very grateful to the patient. This slight change was continued two weeks; but, as it was a mere modification of the milk diet, the latter may be said to have extended over a period of seven weeks.

At the end of seven weeks, and as an *addition* to the milk and curd diet, still continued, a dinner was allowed, consisting of about three-quarters of a pound of beef or mutton roasted, or steak or chop, with a moderate quantity of green vegetables (cabbage, greens, lettuce, spinach, &c.) This change of diet did not cause any return of the sugar in the urine, but contributed much to increase the strength of the patient, and was very grateful to his appetite.

Since the disappearance of the disease, on the 28th June, up the present time (January, 1871), a period of more than six months, the patient has continued and still continues in excellent health, and sugar is still absent from the urine, which is subjected to a weekly examination. Additions have been made to his diet, but all articles containing starch and sugar have been excluded as far as practicable. At present he is taking the following diet daily: — For breakfast: ½lb. of mutton chop, a pint of milk, and about ¼ pint of coffee. For lunch: ¼ lb. of potted head or potted meat and a pint of milk. For dinner: about ¾ lb. of roast beef or mutton, chop or steak, fowl or turkey, with green vegetables (brussels sprouts, cabbage, &c.) After dinner. up to bedtime, tea and a liberal quantity of milk are taken. Six pints of milk are consumed daily; from this the greater portion of the cream has been separated. Under this diet the health of the patient is excellent, and he has gained considerably in healthy flesh, the obesity and flabbiness having quite disappeared. This dietary will be continued for some time longer, until it is considered safe to introduce articles of diet containing starch or sugar.

*Case* 2.—*Diabetes mellitus; removal of the sugar in twelve days; complete recovery.*—Mr. D. S., a highly respectable and prosperous tradesman in Newcastle-on-Tyne, of robust, muscular build, and of temperate habits, but much confined within doors by his business. His health had generally been good until

January, 1870, when he began to suffer much from general indisposition, and when he placed himself under a medical practitioner, who ascertained that he was suffering from diabetes, but did not prescribe a dietetic treatment, or produce any amelioration of the condition of the patient.

On April 15th he consulted an experienced surgeon in Newcastle. At this time his weight was 11 st. 10 lb., and he passed daily from six to seven pints of urine having a specific gravity of 1040 to 1045. He suffered from dry skin and excessive thirst (not greatly increased appetite), great dimness of vision, and almost complete loss of sleep. He had a dull, heavy, aching pain over the loins, great listlessness, entire loss of energy, and experienced great fatigue on the slightest exertion.

The patient was now placed under Rollo's dietetic treatment, consisting of animal food—beef and mutton,—of which he partook about 2½ lb. daily in four meals, with the addition of two or three of Camplin's bran biscuits to each meal; tea was allowed, and also a pint of claret daily, and occasionally a little brandy and cold water. Bread, sugar, and all food containing starchy matter, were strictly prohibited.

On the 19th April (four days after commencing this treatment) his weight was 11 st. 5 lb.; and on the 25th, 11 st. 10 lb.

The effect of the treatment was to diminish the daily quantity of urine to 4½ pints on an average; but its specific gravity never fell below 1038, and continued to be abundantly impregnated with sugar. The patient, however, experienced considerable relief from the general symptoms of the disease up to the 25th of July; but the disease itself showed no indication of yielding, and was pronounced incurable. At this period he became much worse, there being a great aggravation of all the symptoms of the disease up to the 3rd of August, when he first came under my observation.

When, at the date just mentioned, he consulted me, I found the patient suffering from all the symptoms already detailed, and from which he was suffering when placed under a dietetic treatment in the beginning of April. He was passing from six to seven pints of urine daily. The specific gravity on the 3rd of August was 1040.

On the 4th of August, the day following, I placed him under the skim-milk treatment. He was allowed six pints daily, from which the cream had been carefully separated, after standing eighteen or twenty-four hours in a cool place. *All other food was strictly prohibited*, and stimulants were disallowed.

*Report.*—Aug. 3 (day before the treatment was begun). The urine was loaded with sugar. There was great thirst, a dry skin, no sleep at night, loss of energy, and great fatigue on exertion. Quantity of urine 6 pints, sp. g. 1040.

5th (the day after the commencement of the treatment). Not

much improvement in general symptoms; but the urine was reduced 2¼ pints in quantity and ten degrees in specific gravity.

8th. The quantity of sugar in the urine very much diminished; *thirst gone;* skin moist and perspiring, especially at night. Had slept soundly on the previous night; listlessness much less, and feels as if he had got rid of a load. 4¼ pints, sp. g. 1013.

10th. Much less sugar in urine. A great improvement in every respect. 4¼ pints, sp. g. 1013.

13th. Sugar reduced to a very small quantity. Thirst quite gone since the 8th. Skin continues moist; sleeps soundly every night; vision improved; pain in the loins gone; feels much more energetic and active; takes a long walk daily without fatigue. 3¼ pints, sp. g. 1015.

15th. Only a mere trace of sugar observable in the urine. Continued improvement, especially in strength. 4¼ pints, sp. g. 1011.

18th. Sugar totally absent from the urine; still improving in strength and energy; has no desire for more food. 3¼ pints, sp. g. 1016.

20th. Sugar continues absent; "feels quite well;" can walk a long distance without fatigue. The daily allowance of skim-milk increased to seven pints. 4 pints, sp. g. 1014.

23rd. No sugar; keeping quite well. After consulting me on the 20th he walked to Whitburn, and then to Cleadon Station (about five miles), without fatigue. 5 pints, sp. g. 1010.

29th. No sugar; feels quite well. 5 pints, sp. g. 1010.

31st. As before. 5¼ pints, sp. g. 1013.

Sept. 3rd. As before. Now allowed, in addition to seven pints of skim-milk daily, two pints of the same made into curd by essence of rennet. 3 pints, sp. g. 1019.

6th. No sugar; quite well. Has taken much exercise daily, and perspires freely. Bowels constipated, and relieved by castor oil. 2¼ pints, sp. g. 1023.

12th. No sugar; feeling well and strong. At this period the patient was examined, as well as his urine, by my neighbour, Dr. Charles Natrass, of Sunderland, who considered him cured of the disease. The patient, moreover, declared to Dr. Natrass and myself that, on the 6th of Sept., he had walked from my house by the sea-shore to South Shields, a distance of nearly eight miles, without fatigue, and after having lived on skim-milk solely for forty-one days. He further declared he could not have done this feat three years previously, when in health and living generously. 3¼ pints, sp. g. 1019.

15th. No sugar in the urine; weight 11st. 10lb.; health excellent. The patient had now lived solely on a skim-milk diet for a period of thirty-two days, and on skim-milk and curds solely for an additional twelve days, in all forty-four days (six weeks and two days).

On Sept. 16th he was allowed, as an *addition* to the skim-milk and curd diet, half a pound of mutton chop to dinner.

Sept. 19th. No sugar in the urine; patient continuing well; takes daily, with the chop, five pints of skim-milk in the liquid form, and from two to three made into curd. The change in the diet diminished the quantity of urine and raised its specific gravity. 3 pints, sp. g. 1020.

23rd. Urine free from sugar, but containing a deposit of uric acid crystals. 2¼ pints, sp. g. 1026.

27th. Patient perfectly well; has been drinking more milk; allowed the green part of cabbage, greens or lettuce to dinner, with butcher's meat. 4½ pints, sp. g. 1017.

30th. As before. 2¾ pints, sp. g. 1024.

Oct. 3rd, 10th, 23rd, and 29th. No sugar in the urine; health excellent; has been taking much out-door exercise; now allowed Van Abbott's gluten bread to dinner, as an addition to the food last mentioned. 3½ to 4½ pints, sp. g. 1024, 1015.

Nov. 2nd and 10th. No sugar in the urine; health excellent. It was now considered unnecessary to keep a regular record of the case. 3½ to 4 pints, sp. g. 1024, 1018.

Since the above date the patient has been constantly under my observation, and a weekly examination of the urine has been made; and at the period I now write—January, 1871,—nearly seven months after the patient's recovery, the disease has not returned, and the patient is in excellent health, notwithstanding the fact that he has partaken daily of a mixed diet, though carefully regulated to exclude, as far as possible, starch and saccharine matter, and also fat, except to a small amount. The dietary on which he was placed, and on which he is now living, is as follows:—Five to seven pints of skim-milk daily (two or three pints of it converted into curd by essence of rennet); eggs (occasionally a portion of the curd and of the eggs are made into puddings); tea and coffee, with skim-milk; a meal of roast beef or mutton, chop or steak, turkey or chicken, with green vegetables, such as the green part of cabbage, greens, brussels sprouts, lettuce, spinach, &c. Tea has been taken in the evening, and supper has consisted of curds and milk. With this diet the patient is vigorous and quite contented.—*Lancet, May* 6, 1871, *p.* 603.

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## 125.—ON A SERIES OF CASES IN WHICH CHANCRES (?) HAVE FOLLOWED VACCINATION.

By J. HUTCHINSON, Esq., Surgeon to the London Hospital.

*(Read before the Royal Medical and Chirurgical Society.)*

On the 7th February thirteen persons (young adults) were vaccinated from the arm of a healthy-looking infant. All, ex-

cept one, had normal vaccine vesicles, which healed well. In all, except two, indurated chancres have since developed in the vaccination scars. In nearly all, the scar began to inflame and harden during the fifth or sixth week. Several of them have two or three chancres.

The infant (vaccinifer) now has condylomata at the anus, and is beginning to waste (age six months). She is undoubtedly the subject of inherited syphilis, the taint having been latent at the time of vaccination.

None of those vaccinated have as yet shown any persistent secondary rash, but several have been feverish for a day or two, and have had transitory roseola. In all, the sores are disappearing under mercurial treatment.

The two who escaped the syphilitic contagion were the two first vaccinated, and probably they received pure lymph, whilst the others received blood as well. It is known that the vaccinifer's spots bled during the vaccination.

The inferences from the cases were—1. That the blood of a child in the latent stage of inherited syphilis is capable of producing primary syphilitic sores in its recipients, and is indeed remarkably efficient as a means of contagion. 2. That the vaccine virus itself, even when taken from a syphilitic subject, produces nothing but the true vaccine disease. 3. That the two poisons may be conveyed (in two fluids) at the same time, and may each produce its specific effects.

Dr. BAKEWELL called attention to the possibility that syphilis might be conveyed in vaccination not by blood only, but also by the mingling of epidermic scales from the vaccinifer with the lymph. He suggested also that the vaccinifer might receive syphilis from the vaccinated if the lancet that had drawn blood from the latter was introduced into the vesicle to obtain its charge of lymph.

Mr. HENRY LEE thought Dr. Bakewell's suggestion about the possible conveyance of syphilis by epithelium scales was well worthy of attention. He commended Mr. Hutchinson's courage in bringing these cases before the Society. In this country there was much difference of opinion about the production of disease by vaccination; and while many disbelieved that syphilis could be communicated by vaccine matter, their incredulity was confirmed by the official reports. There was, however, a very general belief that impure blood would be injurious; and it was very desirable that the conditions under which harm could be done should be fully made known, in order that medical men might be protected from undeserved obloquy. Mr. Hutchinson had said that if nothing but vaccine lymph were taken, even from the arm of a syphilitic child, nothing but cow-pox could be communicated. He himself would go still further, and

would say that if small-pox and cow-pox were co-existent in the same subject, only cow-pox would be communicated by lymph from the vaccine vesicle. There could be no doubt, however, that vaccination might call previously latent disease into activity; and in this way it might appear to convey syphilis when it had not done so in reality. He had seen three cases in which syphilis was supposed to have been communicated by vaccination; and he thought induration of the axillary glands unusual, although possibly to be accounted for by the part of the arm on which the vaccination had been performed.

Mr. De Meric joined with the last speaker in congratulating Mr. Hutchinson upon his courage in bringing forward the facts; and thought that the paper would change the opinions of many persons who had previously been incredulous about the conveyance of syphilis by vaccination. He thought, however, it would have been better to have delayed the publication for a few weeks, in order to see whether any secondary symptoms would have appeared in the vaccinated persons. He did not believe that it would be safe to use the vaccine lymph yielded by a syphilitic subject; because the vaccine lymph was formed from the blood, and might therefore possibly become a channel for the communication of disease. At the same time, it must be remembered that the sores formed by vaccination sometimes assumed various unusual appearances; and it should not be hastily concluded that these were due to syphilis. There could, notwithstanding, be no doubt that syphilis might be communicated by vaccination; and such an occurrence had taken place in Brittany. The facts had been investigated by a commission of the Paris Academy of Medicine, and had been placed beyond all question.

Mr. Brudenell Carter said that the canon of medical orthodoxy, as regarded vaccination, had hitherto been founded upon three cardinal points of belief. These were, that the relation between the vaccinifer and the vaccinated was the only human relation, that the transference of vaccine lymph was the only mode of human intercommunication, and that the vaccine lymph itself was the only human secretion, by which syphilis could not be conveyed. For his own part, encouraged by the well-known maxim of philosophy, that the multitude of people who believe in a thing do not in the least add to its credibility, he had long had the hardihood to dissent from all three propositions, and he thought that the first two, at least, had now been shown to be utterly untenable. Mr. Hutchinson still gave the weight of his authority to the last; but in doing so he surely rested a very wide generalisation upon a slender basis of fact. It was difficult to discover any reason for the distinction so much insisted upon between blood and lymph; and it was the

practice of many vaccinators, after having taken the first yield of a vesicle, to wait for a few minutes until another drop exuded, a drop that could be nothing but liquor sanguinis, containing all the essential constituents of the blood, and also carrying the peculiar morbid products of the vesicle. Moreover, a few years ago, when he was in the habit of vaccinating, and occasionally sent for tubes of lymph to the Privy Council, the liquid supplied to him was always turbid, often inert, and, on one occasion, he believed, had been mingled with saliva and buccal mucus. He attributed the ordinary turbidity to over-filling of the tubes. To his mind, considering the wide diffusion of syphilis, its occasional invaccination was a matter that could only be prevented by a full recognition of the possibility of the occurrence, and by the most stringent precautions with regard to it. The chorus of "*Je n'ai jamais vu*" was alone almost sufficient to prove that those who joined in it had not turned their eyes in the right direction; and was pernicious as a cause of carelessness in the selection of vaccinifers. The public would not receive the full benefits of vaccination until the dangers connected with it were candidly admitted, dispassionately studied, and carefully guarded against.

Dr. DRYSDALE said that he had once vaccinated a syphilitic child, and had given strict injunctions that no lymph should be taken from it. He afterwards heard that it had been taken to a public institution, where his caution, although mentioned, was disregarded, and that several others were vaccinated from it.

Mr. SIMON said that the statements made with regard to the lymph supplied by the Privy Council were such as could not be substantiated. In the course of a speech of more than half-an-hour's duration he repeated the already published history of his circulars of inquiry about the invaccination of syphilis; and threw out the suggestion that the products of inflammation might be sources of danger, and that hence the use of tenth-day lymph, not infrequent upon the Continent, might be a reason why there had been more evidence of the communication of syphilis abroad than in England.—*Lancet, May* 6, 1871, *p.* 613.

At a meeting of the Committee appointed to investigate this subject—

After detailing the cases of vaccinal syphilis, Mr. JONATHAN HUTCHINSON expressed his faith in vaccination in the strongest terms. He informed the Committee that the danger of vaccinal syphilis was infinitesimally small, that he had vaccinated all his own children, and should continue the practice. He was of opinion that the stringency of the law should not be relaxed; but that it should be an object of legislation to surround the operation with every possible safeguard. Amongst other pro-

visions for preventing the introduction of syphilis, he would prohibit the taking of lymph from any first-born child, and recommend that it should in all cases be taken from second or later children, in families of which the eldest had enjoyed a good state of health. This would reduce the probability of transmitting syphilis by vaccination to the very lowest point.—*Lancet, May* 13, 1871, *p.* 664.

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## 126.—ON THE CASES OF ALLEGED VACCINO-SYPHILIS.

By the EDITOR OF THE LANCET.

In the first place we would urge that, from a scientific point of view no less than from considerations of expediency, it is necessary to require the production of very stringent proofs before admitting the syphilitic character of the sores produced by these vaccinations. As to expediency, we need hardly say that it is a most serious, and if needlessly done a most criminal, act, to let loose among the public the terrorising idea of possible syphilisation through vaccine. The example of Paris is before our eyes. The French scientific *dilettanti* frittered away the confidence of the public in vaccination by long-winded debates on a supposed epidemic of vaccino-syphilis, which turned out to be a complete mare's-nest. And the consequences have been terrible: vaccination was completely neglected, and small-pox has scourged Paris in a manner that reminds one of the mediæval plagues. That a very similar series of events would occur in this country if once the official declaration of a representative Medical Society confirmed the existence of a genuine vaccino-syphilis, we can have little doubt from the mischief which has been already accomplished even by the rash and ignorant assertions of a few medical men who possess not the least scientific knowledge on the subject. All this is no reason for concealing the fact of vaccino-syphilis, if fact there be. But it does throw a most heavy responsibility, which we feel it our duty to point out clearly, on those who bring forward alleged instances of the disease; and the least vestige of scientific levity, or of a tendency to accept inadequate proof, ought to be visited with the sharpest reprobation.

So much for the question of expediency; and now as to the question of fact. We are not aware that, so far, any decisive evidence has been produced that could prove the sores in question to be syphilitic. For our own part, we know of *no characters whatever* that would prove the syphilitic nature of any sore on the arm following an irritant wound, unless there were clear constitutional symptoms of syphilis. The most experienced syphilogue in the world has no right to affirm, from the mere aspect of any sore following an irritant wound, that it is

syphilitic. Such sores are constantly observed, presenting the hardened base and the peculiar margin which are seen in the indurated chancre, and cicatrising precisely in the form of the "fungoid" eminence or "bouton" which follows the true syphilitic sore; and yet the observer knows that the primary lesion could not by any possibility have been syphilitic, and he finds that no constitutional symptoms follow it. Such was the nature, most unquestionably, of the sores produced by those unhappy vaccinations in Brittany in 1866, which caused such a terrible hubbub, and produced the disastrous debates in the Acedémie de Médecine. And we have been independently informed by three different London practitioners, who have been largely engaged in vaccinations during the past six months, that a comparatively common phenomenon of vaccination has been the formation of an extremely ugly-looking sore, which exactly resembled a syphilitic chancre, both in the open and the healed state, but which (out of some score of cases) entirely and perfectly subsided without a vestige of evil result, though several months have now elapsed. This is only a corroboration of what we ourselves have repeatedly witnessed in former days. Now, at present, Mr. Hutchinson's cases have presented no more diagnostic phenomena than the above, *plus* a certain amount of roseola (of itself by no means distinctive) and a doubtful amount of "headache and pains in the limbs" in two or three cases. Assuredly, something much stronger than this is needed! and we hold that the Medical and Chirurgical Society will gravely fail in its duty if it admit the syphilitic nature of these affections without a far more cogent proof.

The other point, to which we would draw the attention of the general profession and of the public as strongly as that of the Medico-Chirurgical Society, is this: that, even supposing it shall ultimately appear that very rare instances of true syphilis from vaccination do occur, there is every probability that the accident would be found to depend on the accidental admixture of the blood of the vaccinifer with the lymph. Such an occurrence is the result of culpable carelessness, and will become impossible when once a proper amount of knowledge and a sufficiently conscientious spirit of carefulness have been diffused among the profession. Even if it does at present with extreme rarity occur (which is very far indeed from being proved), it affords no argument whatever against vaccination; it simply illustrates that want of perfect education as to the requisites for first-class vaccination which makes a few practitioners still careless enough occasionally to take up blood as well as lymph from the vaccinifer. The danger, even if it exists, is already infinitesimal, and will shortly be removed altogether.—*Lancet, May* 6, 1871, *p.* 618.

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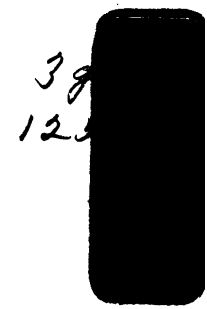
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